

**Opportunities of Cross-fertilization between
Law and Experimental Economics**

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I. Introduction

This dissertation illustrates perspectives that allow cross-fertilization between law and experimental economics. It does so in three separate research articles. Commonly law and economics is understood as a discipline applying (micro) economic methodology to legal questions. These questions can either be policy questions or questions of interpretation. The three Chapters reported below show that legal scholarship and economic research can indeed mutually inspire each other in both directions. That means that apart from answering legal questions by means of economics (Chapter II), legal scholarship (Chapter III) as well as knowledge about legal practice (Chapter IV) can nudge economic thinking into directions economic theory seems reluctant to go.

Chapter II sets out where one would expect law and economics to be fruitfully applied: answering legal questions with economic methodology. In antitrust, the legal question arose whether target rebates can foreclose markets inefficiently because they exploit non-standard preferences of consumers. Chapter II is answering a part of this question by testing whether loyalty rebates can impose psychological switching costs on consumers.

Loyalty consumer rebates are a common marketing device. The consumer loyalty rebates treated here (also referred to as ‘target rebate’ or ‘all unit discount’) are based on the following mechanism: A firm grants a significant price reduction on all units bought during a certain reference period if within that reference period the customer reaches a certain target in purchases close to his total demand. Thereby, the target is framed as a goal the consumer aims for.

Loyalty rebates could induce consumers to adopt such goals. The goal may lead consumers to interpret foregoing the rebate as a loss inducing risk seeking behavior to assure reaching the rebate threshold. This may translate into psychological switching costs (for the basic mechanism see Kahneman & Tversky, 1979).

The relevance of non-rational behavior in competition policy has been vividly discussed in the antitrust community under the label “behavioral antitrust” (Tor & Rinner 2011; Stucke 2007; Reeves & Stucke 2011; Tor 2002). This new direction of research received a lot of attention and support (see, e.g., a special issue of Competition Policy International in 2010; the conference web site <http://behavioralantitrust.acle.nl>; the speech by Federal Trade Commissioner Rosch, 2010), but it was also criticized for prematurely applying insights derived from a student subject pool to firm behavior, for pursuing a paternalistic agenda, and for making welfare analysis impossible (Werden, Froeb, & Shor

2011).

The most promising applications of behavioral antitrust have dealt with consumer behavior. Replacing a standard demand function with a more realistic model of consumer behavior often leads to very different predictions in situations highly relevant to antitrust (see the survey report to the British Office of Fair Trading by Huck et al. 2011). The criticism of extrapolating insights from observed behavior of participants in lab experiments (most of whom are students) to corporate behavior cannot be leveled when thinking about consumer behavior because students are typically consumers in many markets. Additionally, the paternalism argument has less bite in consumer protection contexts because consumer protection law specifically aims at protecting consumers where they cannot protect themselves. Finally, that behavioral antitrust makes welfare analysis impossible is not a convincing argument against behavioral antitrust per se. On the one hand, where one needs to predict agents' behavior in markets one should certainly use the best performing model. In contexts where "behavioral" models outperform rational choice in predicting peoples' behavior one should use "behavioral" models. On the other hand, even where normative inferences are derived from welfare analysis one cannot simply reinterpret mistakes people make (e.g. by responding to mere framing) as revealed preferences. Such a procedure would turn welfare analysis uninformative about agents' well-being. Clinging to uninformative welfare analysis just because it is possible boils down to ignoring the problem. In fact the argument that behavioral antitrust renders welfare analysis impossible merely urges the discipline of economics to meet the challenge of constructing welfare under endogenous preferences (Morell 2011 for more detail).

We predict psychological switching costs on the basis of cumulative prospect theory (CPT). Our experiment sets up a minimal rebate paradigm focusing on the very essentials. We do not use any factor, which improves the psychological attractiveness of a rebate beyond the pure conditional monetary payoff structure. We pursue this minimal rebate paradigm to generate reliable evidence that indeed the mere payoff structure suffices to generate the observed effects.

From the three experiments reported in Chapter II we conclude that loyalty rebates lead to non-rational buying behavior, amounting to an additional psychological switching cost that can cause substantial financial losses for consumers. This effect increases the potential of loyalty rebates to be used as a tool to foreclose markets and provides an argument for a more restrictive position towards loyalty rebates under consumer protection law. Previous arguments and rulings concerning the regulation of loyalty rebates under antitrust law both in the EU and in the US were mainly based on the assumption of rational buying. Stickiness effects add to these existing problems. Therefore the

potential danger of loyalty rebates has been underestimated. The demonstrated stickiness effect backs the role psychological effects already play in European antitrust law today. It generally supports the greater scrutiny loyalty rebates have recently been subject to both in the EU and the US. Our results have been cited and confirmed by Bruttel (2013).

Chapter III goes beyond the standard law and economics approach of answering a legal question employing methods from economics. It illustrates that the reverse direction of inspiration is possible too. This chapter deals with “guilt aversion”, a theory postulating second order beliefs to induce action (Battigalli & Dufwenberg, 2007; Charness & Dufwenberg, 2006). The paper answers the question whether second order beliefs induce action stronger across low social distance than across large social distance. This question is genuinely economic. It arises from a branch of literature postulating and testing second order belief dependent preferences with mixed empirical results. The lab-career of guilt aversion started promising (Charness & Dufwenberg, 2006) further tests of the theory of guilt aversion in the lab led to mixed results. Vanberg (2008) conducted a dictator game experiment where he does not observe any causal effect of second order beliefs on action. Reuben et al. (2009) conduct an experiment in which they elicit investors’ beliefs in a trust game and report them to trustees. They do find evidence in favor of guilt aversion. Ellingsen et al. (2010) conduct a series of dictator game and investment game experiments where receivers report their beliefs on the amount sent to the experimenter and the experimenter reports these beliefs to the senders, inducing second order beliefs. They find evidence that second order beliefs do not determine action but that actions induce second order beliefs. Lately even prominent promoters of guilt aversion, Charness and Dufwenberg (2010), merely found “limited support” for guilt aversion. And finally, in a trust game with an investor, a trustee, and two inactive players Bellemare et al. (2011) found trustees to have a positive willingness to pay to avoid guilt vis-à-vis the investor only. At first glance, the literature could lead the reader to believe that the correlation between second order beliefs and actions is an instable phenomenon that tends to be revealed as a confound – either with a preference to keep a promise (Vanberg, 2008) or with a (false) consensus effect (Ellingsen et al., 2010).

Here legal thinking facilitated fruitful amendments of economic theory. The literature of guilt aversion has been applied to answer the question why people hold promises to a stranger. Lawyers have been immensely interested in why and when people hold promises because in legal history the moral obligation of holding a promise in canon (i.e. religious) law an important root of modern continental contract law. Lawyers have long seen promises to change the relationship (mostly and vaguely understood as a complex set of norms an indi-

vidual perceives to connect her to another person) between people. Lawyers do speak of “contractual relationships” and mean what they say when using the term relationship. In Germany many obligations US law treats under torts have been reinforced by moving them into contracts by arguing contractual parties would assume a special obligation to care for they partner’s private interests. This focus on relationship facilitates seeing the significance of social closeness in the existing economic literature triggering the theoretical amendments and the experimental test set out in chapter III. Re-analyzing the experiments against this legal backdrop with regard to the intensity of the relationship between subjects reveals the hunch that indeed experiments using a protocol that allows for social closeness generate results in favor of guilt aversion while those experiments using a protocol of anonymity do not (see Charness and Dufwenberg (2006), Reuben et al. (2009), Bellemare et al. (2011) on the one hand and Vanberg (2008), Ellingsen et al. (2010) Charness and Dufwenberg (2010) on the other).

I assess the impact of social closeness on guilt aversion in a dictator game by systematically varying it using a minimal group paradigm. I find that indeed in interactions between members of the same group second order beliefs induce action more strongly than in interactions of members of different groups.

My findings are relevant for long studied unresolved questions: They suggest that people may keep unenforceable promises because the promise establishes a closer relationship between the parties. This may reconcile or amend the two competing explanations discussed in the literature so far (Gary Charness & Dufwenberg, 2006; Vanberg, 2008). Maybe it is not astonishing that the sole other paper proposing a similar two step mechanism of promise keeping was co-authored by a lawyer-economist.

My results also suggest that different levels of second order beliefs alone cannot explain ingroup favoritism (Güth, Ploner, & Regner, 2009; Ockenfels & Werner, n.d.). Rather second order beliefs matter more in ingroup interactions. My main finding that shared identity induces the influence of second order beliefs on action explains why guilt aversion has been rejected in anonymous experiments while it has been confirmed in experiments allowing for some form of relationship between participants. Thereby it structures the so far inconclusive literature on guilt aversion, contradicts conclusions that guilt aversion has been rejected in its entirety (Ellingsen, Johannesson, Tjøtta, & Torsvik, 2010), and specifies the realm of application of guilt aversion to social interaction across small social distance.

In **chapter IV** I exploit a different trait of legal scholarship to inspire economic reasoning. Some consider law to be more of an artisanry than a science.

Chapter IV exploits the advantages of legal artisanry for economic science to produce insights beyond the scope of contemporary economic theory. Economic literature has investigated in why and when economic agents organize in partnerships splitting the proceeds earned. With the notable exception of Bornstein et al. (2002, 2008) they ignored the question of oligopolistic pricing of these partnerships. And even Bornstein et al. ignored crucial features of oligopolistic competition of partnerships. For a legal artisan these features are obvious because they characterize the oligopolistic competition between elite law firms. This provided the motivation of an experiment that consciously goes beyond testing existing theory because no theory predicted our treatment manipulations to make any difference. But the professional hunch of a legal artisan let these manipulations seem promising.

Agents in economic models are typically modeled as monolithic decision makers. While this is a useful simplifying assumption, it is in contrast with the observation that many real world economic decisions are made by groups or teams. Boards of directors determine the behavior of firms, and self-monitored profit-sharing teams characterize many industries for artistic and professional services, e.g., lawyers, consultants, or music bands.

Modeling economic agents as teams is important because team dynamics and decisions, and subsequently the outcomes of inter-team interactions and markets in which teams operate, can be crucially influenced by the teams' internal organization. In this paper we experimentally study how team's internal organization, operationalized as the way profits are divided among team members, affects the unfolding of duopoly Bertrand competitions.

Suppose that a state agency wishes to sue a construction company, claiming back aid granted for the construction of a power plant. The state decides to auction off the mandate in a public procurement auction, where the applicant who submits the lowest asking price wins the auction. Applicants need to bring in expertise in dispute resolution because it is a court case; the embeddedness of the case in the energy sector requires expertise in energy law; and finally the core of the case certainly lies in the law of state aid. Since such a wide array of qualifications is beyond the scope of any single lawyer, a team of lawyers submits a joint tender, competing against other teams, and the team with the lowest asking price wins the project and is paid its asking price.

A straightforward way to model the pricing decision in the above scenario is to assume that each expert in the team states a personal asking price and the joint bid is the sum of these asking prices. Clearly, team members have a joint interest in winning the project by asking for prices that are lower, in sum, than

those of the competitors. However, depending on the way profits are divided among members of the winning team, they can also have conflicting interests; given the public good flavor of low joint bids, if team members receive their own personal bids when the team wins, then each one would prefer her teammates to bid low while bidding high herself to maximize her profits.

We consider two ways of dividing profits among members of the winning team in a Bertrand price competition: (1) each member of the winning team receives her personal asking price; and (2) each member of the winning team receives the average personal asking prices in the team (an equal share of the team's profit). It is clear that the internal conflict within the team is pronounced in (1) and absent in (2). Relating these two incentive structures to the example above and to real teams in the legal domain, two (stylized) types of competitors can apply to take the mandate: *consortia* and *partnerships*. Both consortia and partnerships are composed of a number of lawyers, each with one of the required specializations. In a consortium it is common that each lawyer fixes her own hourly rate, and is paid – in case the consortium gets the mandate – according to this rate. On the other hand, in partnerships the usual practice is that all partners agree on identical hourly rates for all partners, and some even place yearly profits in a pot to be distributed equally or at least at fixed ratios among all partners at the end of the year.

We experimentally examine both homogeneous duopolies composed of either two consortia or two partnerships, and heterogeneous duopolies composed of one consortium and one partnership. Additionally, we vary the transparency of the profit sharing arrangements, i.e., whether team members have information about the profit sharing method of the other team or not. In summary, our results show that (1) Homogenous consortia markets yield substantially higher prices than homogeneous partnership markets, both when profit sharing arrangements are transparent and when they are intransparent. (2) When profit sharing arrangements are transparent, prices in heterogeneous markets are as low as prices in homogenous partnership markets. (3) When profit sharing arrangements are intransparent, prices in heterogeneous markets are (almost) as high as prices in homogenous consortia markets. (4) Transparency of profit sharing arrangements leads to higher prices in homogeneous markets but to lower prices in heterogeneous markets.

Our results can assist teams in forming preferences about their own profit sharing rule and transparency policy and about the types of markets they choose to compete in, as well as inform market regulators in the design of trading institutions (as all determinants of price levels are relevant for the effi-

cient distribution of goods) or in forecasting (tacit) collusion, which is usually thought of as being attained by coordination on prices, but may also be attained by coordination on less competitive internal structures.

In the next section we relate our work to existing literature. Then we present our experimental design and procedures. Subsequently we derive testable hypotheses. Section four presents the results of the experiment. Section five concludes the paper and provides recommendations for market participants and policy makers.

So while chapter II represents an example of the well-established interdisciplinary cooperation between law and economics where law turns to economics in search of answers to legal questions, chapter III and IV represent examples of economic research guided by legal knowledge or intuition.

Chapter II is a joint paper together with Andreas Glöckner and Emanuel Towfigh. Chapter IV is a joint paper with Michael Kurschilgen and Ori Weisel. My contributions to the two papers are as represented in the following table.

Table I: Personal contribution to co-authored Chapters		
	Chapter II	Chapter IV
Idea	Leading	Leading
Experimental Design	Leading	Proportional
Hypotheses	Leading	Leading
Literature Review	Leading	Leading
Data Collection	Proportional	Leading
Data Analysis	Proportional	Proportional
Writing	Leading	Leading

II. Sticky Rebates: Loyalty Rebates Impede Rational Switching of Consumer

1. Introduction

Loyalty consumer rebates are omnipresent. The average US household is reported to participate in 6.2 loyalty programs (Dreze & Nunes 2011). Many of these loyalty programs imply a conditional element. Retailers (e.g., Best Buy, Anson's, Peek & Cloppenburg), hotel chains, and airlines offer discounts, preferential service, premiums, or extra bonus miles conditional on the consumer purchasing a certain minimum per year. This paper shows that these conditional loyalty rebates are prone to creating psychological switching costs in consumers, rendering them a potential threat to competition.

The consumer loyalty rebates treated here (also referred to as 'target rebate' or 'all unit discount') are based on the following mechanism: A firm grants a significant price reduction on all units bought during a certain reference period if within that reference period the customer reaches a certain target in purchases close to his total demand. Thereby, the target is framed as a goal the consumer aims for. A couple of years ago, Lufthansa, the largest German airline, offered its customers a particularly clean example of a consumer loyalty rebate scheme. Customers received a discount (i.e., further bonus miles) on all purchases within a year (i.e., reference period), if they reached a threshold close to their expected demand during that year.¹ The first sentence Lufthansa wrote to its customers when introducing the new conditional rebate was: "Dear Mr./Mrs. X, do you know the marvelous feeling of having reached a goal you set yourself?" In the current paper we investigate whether loyalty rebates that induce consumers to adopt such goals, be they imposed or self-set, pose a threat to competition by imposing additional switching costs. Specifically, as explained in detail below it can be expected on theoretical grounds that goals shift reference points upwards so that foregoing the rebate is perceived as a loss. According to Prospect Theory (Kahneman & Tversky, 1979, details below), this should make individuals more reluctant to switch to a different supplier due to

¹ Here, heterogeneity of consumers posed a serious problem for setting a unified threshold close to expected demand, which Lufthansa solved by inciting consumers to set their own target for the year to come. Other suppliers solve the same problem by offering several targets that yield increasing rebates.

loss aversion, leading to a psychological increased switching cost. The switching cost can ultimately have detrimental effects on competition, which should be taken into account in the legal assessment and regulation of rebates. There is empirical evidence, for instance, that an airline dominating a hub airport can use frequent flyer programs to foreclose smaller but equally efficient competitors from the market (Lederman 2007). The psychological switching costs that target rebates generate could reinforce or even cause this effect. The potential of target rebates to inefficiently foreclose markets makes loyalty programs a potential issue of antitrust law prohibiting dominant firms to abuse their market power (Sec. 2 Sherman Act [ShA] and Art. 102 Treaty on the Functioning of the European Union [TFEU]). One may furthermore consider action under consumer protection laws, as the described psychological switching cost is to the detriment of consumers.

The relevance of non-rational behavior in competition has been vividly discussed in the antitrust community under the label “behavioral antitrust”: Retail price maintenance (Tor & Rinner 2011) merger control (Stucke 2007; Reeves & Stucke 2011) and market entry (Tor 2002) for example, have been reevaluated using insights from behavioral economics. This new direction of research received a lot of attention and support (see, e.g., a special issue of *Competition Policy International* in 2010; the conference web site <http://behavioralantitrust.acle.nl>; the speech by Federal Trade Commissioner Rosch, 2010), but it was also criticized for applying insights derived from a student subject pool to firm behavior, for pursuing a paternalistic agenda, and for making welfare analysis impossible (Werden, Froeb, & Shor 2011).

The most promising applications of behavioral antitrust have dealt with consumer behavior. Replacing a standard demand function with a more realistic model of consumer behavior often leads to very different predictions in situations highly relevant to antitrust (see the survey report to the British Office of Fair Trading by Huck et al. 2011). The criticism of extrapolating insights from observed behavior of participants in lab experiments (most of whom are students) to corporate behavior cannot be leveled when thinking about consumer behavior because students are typically consumers in many markets. Additionally, the paternalism argument has less bite in consumer protection contexts because consumer protection law specifically aims at protecting consumers where they cannot protect themselves. Finally, that behavioral antitrust makes welfare analysis impossible is not a convincing argument against behavioral antitrust per se. On the one hand, where one needs to predict agents’ behavior in markets one should certainly use the best performing model. In contexts where “behavioral” models outperform rational choice in predicting peoples’ behavior one should use “behavioral” models. On the other hand, even where normative inferences are derived from welfare analysis one cannot simply rein-

interpret mistakes people make (e.g. by responding to mere framing) as revealed preferences. Such a procedure would turn welfare analysis uninformative about agents' well-being. Clinging to uninformative welfare analysis just because it is possible boils down to ignoring the problem. In fact the argument that behavioral antitrust renders welfare analysis impossible merely urges the discipline of economics to meet the challenge of constructing welfare under endogenous preferences (Morell 2011 for more detail).

Rebates are high on the agenda of competition policy both in the US and the EU. In both jurisdictions, a tendency is emerging to consider the psychology of buying behavior in practice.

US Courts used to take a rather lenient position towards loyalty rebates, in particular if they only concerned one product (see, for example, the case *Concord Boat v. Brunswick*). Although single product rebates long seemed to be legal per se, recent cases like *AMD vs. Intel* or *ZF Meritor vs. Eaton* have shown that conditional rebates can lead to expensive settlements or even to antitrust liability under Sec. 2 Sherman Act. American Courts start to worry about the potential of conditional rebates to serve as substitutes for exclusive dealing arrangements and to foreclose markets inefficiently.

Even though we are not aware of any US antitrust decision or opinion explicitly referring to any psychological state of mind, we understand the Supreme Court's distinction between 'sophisticated' and 'unsophisticated' consumers to point into a similar direction. Information cost can be both, organizational or cognitive. And psychological effects contribute greatly to cognitive costs of information. In *Kodak*, the Supreme Court treats behavior of unsophisticated consumers to be relevant insofar as it affects markets. Consumers who are prone to experience psychological switching costs could just be categorized as a subtype of unsophisticated consumers.

Even in the single product case, European antitrust authorities have long been concerned about detrimental effects of loyalty rebates generating a discontinuity in the pricing function that may cause a 'suction effect' (see below). But on top of these effects conditional rebates may have on rational buyers, European Authorities now worry about the "weak psychological position" rebates place buyers in (COMP/E-2/36.041/PO — Michelin, no. 224, a case concerning small professional buyers). For both, the standard and the psychological reasons, the European Commission and the European Courts have suppressed loyalty and target rebates with a target close to total demand *per se* if they were applied by a dominant company unless their reference period was shorter than three months (*Hoffmann-LaRoche v. Commission*; *Michelin v. Commission I*; *British Airways v. Commission*; *Michelin v. Commission II*; *Tomra v. Commission*; *Intel v. Commission*). For the future, the European Commission included rebates among its enforcement priorities under Art. 102 TFEU (DG Competition 2005; Eu-

ropean Commission 2009). Furthermore, in case the European Commission is correct in that loyalty rebates even put professional buyers in a weak psychological position, this should suggest that these rebates offered to consumers should *a fortiori* raise consumer protection concerns.

The current paper seeks to provide empirical data for the key questions whether individuals indeed stick to loyalty rebate schemes, even when switching to an outside option (a competitor's product) yields a higher expected payoff and less risk. It also intends to ascertain which factors influence the degree to which rebates create a psychological switching cost.

For this purpose, we investigate the influence of loyalty rebates on consumers' purchasing behavior. Applying Cumulative Prospect Theory (Tversky & Kahneman, 1992) as a candidate model of consumer behavior, we predict and find that conditional loyalty rebates induce a psychological switching cost. By means of these switching costs, conditional rebates are a potential tool for inefficient market foreclosure and may directly harm consumers. With regard to antitrust law and to consumer protection law, our findings provide an argument to intensify the scrutiny rebates are subject to.

2. Effects of Loyalty Rebates

Generally target rebates raise competition concerns because they create switching costs. Starting in the framework of rational choice theory we will first explain how these switching costs arise and how they can lead to detrimental effects on competition. Then we will explain what additional concerns they raise if psychological switching costs are considered.

2.1 Predictions of Rational Choice Theory

From a perspective of rational choice theory (RCT), rebates generate switching costs. If a rebate is granted under the condition of exclusivity, sourcing parts of one's demand from a competitor means foregoing the rebate. Switching therefore comes at a cost. To be attractive, a competitor's offer has to outweigh these costs.

This switching cost is higher per unit if the rebate is effectively distributed only over a small part of demand because either the rest of demand has already been sourced from the incumbent or because the rest will be sourced from the incumbent for sure (assured base of sales). This phenomenon sometimes has been coined "suction effect": The more you have bought the more attractive the rebate — because the full rebate now strongly reduces the price of the small remaining volume of purchases (OECD, 2002; European Commission, 2009).

A rebate's suction effect also may increase as a function of completed purchases if there is uncertainty about the instances of buying opportunities. Purchases may reduce uncertainty about whether a buyer will actually get the rebate. Imagine the buyer may have two purchasing opportunities, each arising with 50% probability and reaching the rebate requires buying at both. Then at the outset the probability of reaching the rebate will be 25% ($50\% \times 50\%$). Once the first purchase has been made the probability will be 50%. This may reinforce the "suction effect" and thus increase switching costs.

Switching costs are not a problem *per se*. But they may lead to inefficient foreclosure under certain market conditions. In analogy to exclusive dealing agreements (Gual et al. 2005) foreclosure by conditional rebates may for instance require uncertainty about the entrant's costs. If then the incumbent uses rebates to extract a "market entrance fee" from the entrant and if he calibrates the fee to the expected costs of an entrant some entrants may be efficient in principle but not efficient enough to afford the entrance fee. Thereby these efficient entrants are foreclosed (Bolton & Aghion, 1987). Also large economies of scale can enable inefficient foreclosure by rebates. Economies of scale may make entry impossible unless the entrant captures enough buyers to reach an efficient scale. Exclusive dealing agreements as well as target rebates can induce coordination failure among buyers preventing the entrant from reaching the efficient scale and ultimately preventing efficient entry (Rasmusen et al. 1991, Bernheim & Whinston 1998). Finally the European Commission (2009) has proposed a test that identifies an assured base of sales ("non contestable share"), which the competitor cannot realistically tackle as a condition to successfully foreclose a market with the help of rebates.

Common to all these approaches of modeling potential harmful effects of rebates is the strategic use of switching costs (penalty, foregoing a discount, foregoing very low prices), which the incumbent controls.

2.2. Predictions of Cumulative Prospect Theory

We argue that from a behaviorally informed perspective, namely from the perspective of Cumulative Prospect Theory (CPT; Tversky & Kahneman, 1992), the switching costs of rebates should, however, go beyond the effects described so far on the basis of RCT. On top of the switching costs predicted by rational choice, rebates should also create psychological switching costs. In the approach of Bolton and Aghion (1987) higher, unpredicted psychological switching costs would unintentionally increase the entrance fee and lead to even more inefficient foreclosure. In the Commission's framework psychological switching costs by which customers refrain from switching to a competitor although he does make attractive offers may stabilize a non-contestable share. While additional psychological switching costs will be relevant in most models

on rebates, in this paper we look at switching costs in isolation. Subsequent work may integrate what we find into market models.

CPT is a theory that applies to decisions under risk. In fact rebates place buyers into a situation of risk. Commonly buyers cannot predict with precision whether they will reach the rebate target or not. Accordingly with some probability they may pay a high and with some other probability they will pay a low price. Buying outside a rebate scheme at a constant price per unit eliminates this risk. But it may increase the expected price in return. A rational risk neutral buyer would certainly switch out of a rebate scheme if the outside option offered a higher expected payoff. If the rational agent was also risk averse – as it is commonly assumed and found in reality (Holt & Laury, 2002) – he would have an additional reason to leave the rebate. In contrast, for a certain parameter space CPT would predict that even if the outside option offers lower risk and higher expected payoff, a buyer would keep buying in the rebate scheme. We will refer to this as the *stickiness effect* of rebates. Our experiment will test these opposed predictions of RCT on the one and CPT on the other hand.

According to CPT, rebates should induce irrational stickiness of consumers due to reference point shifts – on top of the issues already discussed in the literature. Preferences should depend on reference points, which are influenced by hopes (Thaler, 1985; Tversky & Kahneman, 1981; Kahneman & Tversky, 1979), goals (Heath, Larik, & Wu, 1999), and expectations (Abeler, Falk, Götte, & Huffman, 2009). Buyers will hope to reach the rebate and adopt reaching the rebate threshold as their goal. Hence, they will consider a failure to reach the rebate as a loss. In the loss frame, individuals usually seek risk (Kahneman & Tversky, 1979) and are therefore likely to prefer the risky option (i.e., stay in the rebate) over a safe outside option with equal expected value or even a higher expected value (i.e. purchase the outside option at a constant price).

Using standard parameters and assuming that the rebate payoff is adopted as the reference point, in Appendix A we formally derive from CPT the prediction that irrational stickiness should be observed for all rebates for which not reaching the rebate is sufficiently likely (i.e., the probability of reaching the rebate must be smaller than 76%; see the fourfold pattern of risk attitudes, Tversky & Kahneman, 1992; see also Schmidt & Zank, 2008; Glöckner & Pachur, 2012). Furthermore, stickiness should increase with increasing magnitude of the rebate, that is, the difference between the overall payoffs for

reaching vs. not reaching the rebate. Finally, when taking into account individual differences, stickiness should increase with increasing loss aversion.²

2.3 Previous Findings

The predictive power of CPT for decision behavior has been supported by ample evidence using student participants (e.g., Glöckner & Betsch, 2008; Glöckner & Pachur, 2012; Kahneman & Tversky, 1979; Tversky & Kahneman, 1992), but also using representative samples of the Dutch population (Booij, Van Praag, & Van de Kuilen, 2010) and “in the wild” (e.g., Camerer, 2005). However, some limitations have also been demonstrated: Using a critical property approach, it has, for instance, been shown that CPT cannot account for several systematic effects in three-outcome gambles (Birnbaum, 2006, 2008a, 2008b). Recent research also indicates that some effects predicted by CPT disappear in decisions from experience (e.g., Erev, Ert, & Yechiam, 2008; Hertwig, Barron, Weber, & Erev, 2004; Hilbig & Glöckner, 2011). Furthermore, process analysis indicates that CPT should not be considered to be a process model for decision-making (Glöckner & Herbold, 2011). Nevertheless, many findings, including the ones mentioned above, suggest that CPT is a reasonable paramorphic (as-if) model for choices in two-outcome prospects with stated probabilities, such as the ones considered in this paper.

In contrast to the large literature on CPT, only a certain branch of marketing research has contributed specifically to empirically exploring the effect of rebates (Dreze & Nunes 2004; Nunes & Dreze 2006a; Nunes & Dreze 2006b; Kivetz, Urminsky, & Zheng 2006; Wirtz et al. 2007). This literature concentrates on optimizing loyalty programs. It lacks contributions showing what the minimum rebate design is that still can impede rational switching and implement substantial psychological switching costs. Our experiment sets up a minimal rebate paradigm focusing on the very essentials. We do not use any factor, which improves the psychological attractiveness of a rebate beyond the pure conditional monetary payoff structure. Given the results from the management

² It should be noted that behavioral effects that go beyond what is captured in CPT, such as routine effects (Betsch, 2005; Betsch, Brinkmann, Fiedler, & Breining, 1999; Betsch, Haberstroh, Glöckner, Haar, & Fiedler, 2001), sunk cost effects (Arkes & Blumer, 1985), or cognitive dissonance (Festinger, 1957; Shultz & Lepper, 1996), might contribute to stickiness effects as well. We will focus our investigation on predictions by CPT, because of its prominence and because, in contrast to the other models, it is sufficiently well specified in mathematical terms to allow predicting choice behavior very accurately also on the individual level (Glöckner & Pachur, 2012). However, we partially take into account these effects to construct strong hypotheses for a critical test of CPT.

literature, our rebate scheme should have a hard time to seduce any participant not to maximize her expected payoffs. We pursue this minimal rebate paradigm to generate reliable evidence that indeed the mere payoff structure suffices to generate the observed effects.

One sole experiment was conducted specifically to feed into the antitrust law and economics of rebates. It demonstrated non-rational attraction effects of loyalty rebates (Beckenkamp & Maier-Rigaud, 2006). For simulated retail markets, Beckenkamp and Maier-Rigaud showed that subjects stuck to a loyalty rebate scheme, even if maximizing the expected payoff suggested otherwise. Although this previous work was important, it addressed relatively complex decisions in retail markets only and had some further limitations that we would like to overcome in the current study.

Regarding theory, Beckenkamp and Maier-Rigaud do not account for the mutual offsetting effects of the value function and probability weighting function of CPT when deriving their hypothesis. And in their experiment, subjects in fact had to solve a newsvendor problem (see Khouja, 1999, for a survey), which most subjects must have considered extremely difficult to do. Because subjects started out in a rebate scheme by default, they may have stayed loyal merely because they wanted to avoid any decision (including the decision to switch) in a situation they felt they did not oversee.

Our approach differs in four crucial respects from that of Beckenkamp and Maier-Rigaud. First, we focus on consumer decisions in contrast to retailer decisions; second, like many consumer environments our experimental tasks are simple, transparent, and easy to grasp and solve; third, we investigate factors possibly influencing the magnitude of the effect based on predictions of CPT; fourth, in our task consumers themselves decided whether to enter the rebate or not so that the rebate was not preset as a default.

3. General Method and Hypotheses

In three experiments with a total number of 175 participants we investigate experimentally whether stickiness can be empirically observed and whether its size can be experimentally influenced. We therefore manipulate the realization of expected demand affecting the relative attractiveness of the rebate scheme vis-à-vis an outside option. We further manipulate the magnitude of the rebate (e.g., overall 10 € rebate instead of 5 € rebate) and investigate the influence of mere buying frequency in the rebate scheme (e.g., buying 10 instead of 5 objects), while holding the differences in total payoffs (rebate magnitude) constant. We thereby stripped down the design to the very essentials of a consumer loyalty rebate scheme setting. In analogy to the abovementioned Lufthansa example, the situation we aim to capture is the following: a consum-

er has the possibility to enter a loyalty rebate scheme for a product he intends to buy repeatedly in a certain time period. If he reaches the imposed³ target (e.g., buying 10 items), the rebate will be granted for all items bought and the overall price will be extremely low; if he does not reach the target, however, the rebate will not be granted and the price will be high. The price of the outside option is between these two prices. After some time, a random event (“external shock”) decreases the likelihood that he or she can reach the target, so that it becomes rational to switch. We measure whether persons switch or stick to the rebate.

We realize this by consecutive buying decisions (*rounds*) concerning tokens connected by a rebate condition. Two chance moves that can lead to the omission of the *critical round* and of the *last round* represent the uncertainty about consumers’ demands. The critical round is omitted with a certain probability. Options are constructed so that according to RCT people should switch to a safe outside option if the critical round is omitted. The chance move in the last round is necessary to maintain uncertainty about consumers’ demand even after the consumer has learned whether the critical round takes place. We manipulate the number of repetitions (*rounds*) of buying and the magnitude of the rebate granted as between subjects’ conditions.

In the experiments, we use rebate schemes with a sufficiently high probability for not reaching the rebate (after the critical round was omitted). As explained above, and as shown in Appendix A, CPT predicts:

Stickiness Hypothesis (H1): subjects who have consistently bought tokens up to the critical round do not exit the rebate scheme even if exit yields a higher expected payoff.

Beyond investigating the mere existence of the stickiness effect, we were interested whether CPT can also predict its severity. We thereby constructed our material to test two further hypotheses, including manipulations for which an effect was predicted and one for which a null-effect was predicted. The second manipulation was also selected to test an assumption underlying core arguments recently used in the regulation of rebates.

³ In the experiment we do not face the difficulty of heterogeneous consumer demand because we can induce it. As we did not want to study the effect of a self-imposed target but that of the essential features of a target rebate we did not let subjects chose their target but imposed it. If anything, imposing the target should work against our hypothesis because participants could be expected to be more reluctant to regard an imposed target as their goal than they would to regard a self-set goal as their target.

According to CPT, the stickiness effect should increase with increasing difference between the total payoffs of reaching vs. not reaching the rebates (see Appendix A). We therefore predict:

Magnitude Hypothesis (H2): A rebate of larger magnitude leads to greater stickiness.

According to CPT, the stickiness of rebates should mainly depend on magnitude, that is, the difference between high and low payoff (see Appendix A). It should not be influenced by the mere number of repetitions of previous buying. CPT therefore predicts the following null-hypothesis:

Repetition Null-Hypothesis (H3): The stickiness of rebates does not increase with the mere number of repetitions of buying if the magnitude of the rebate is constant.

Note that this is a strong null hypothesis. Previous findings indicate increased routine effects with repeated buying (Betsch, et al., 2001), which speaks against the CPT prediction. Additionally, with more repetitions subjects “invest” more money into the rebate. This may trigger a sunk cost effect (cf. Arkes & Blumer, 1985) that also works against the specific CPT prediction. This hypothesis is also particularly interesting for practical reasons, because it captures the claim by the Court of Justice of the European Union that a longer reference period of a loyalty rebate may lead to more market foreclosure (*Michelin v. Commission I.* no. 82; *Michelin v. Commission II.* no. 85). Of course, in the situations referred to by the Court of Justice of the European Union, the number of rounds and the differences between total payoffs will most likely be confounded. It is nevertheless relevant to differentiate between effects of magnitude and repetition.

4. Experiment 1: Sticky Rebates and Indirect Comparison

4.1 Method

4.1.1 Participants and Design

Participants were recruited from the MPI Decision Lab subject pool using ORSEE (Greiner, 2004). The majority of participants were students of the University of Bonn, from a wide variety of subject backgrounds. A total of 64 participants (mean age: 24, 37 female) took part in the 6 sessions. The study lasted between 60 and 90 minutes and participants received a performance-contingent payoff (range: 0.94 € to 17.80 €; approximately USD 1.40 to 26.70)⁴ in exchange for their participation. We use a 2 (negative shock on expected demand: critical round omitted vs. critical round is played) x 2 (repetition in

⁴ These payoffs include the gains and losses subjects incurred when they chose and played the lotteries measuring their risk preferences and loss aversion.

buying: low vs. high) x 2 (rebate magnitude: low vs. high) mixed effects design. The within subject effect of the shock tests the stickiness hypothesis. The between subject effects of rebate magnitude and repetition test the hypotheses two and three respectively. While all subjects go through both demand shock conditions they are randomly assigned to one of the two repetition conditions and to one of the two magnitude conditions.

4.1.2 Procedure

First, participants read the experimental instructions and answered a control questionnaire (see Appendix for both) to ensure that they had understood the instructions and were able to calculate the possible payoffs. Subjects were provided with pocket calculators they could use at any time during the entire experiment. The main instructions are given in Appendix B. Payoffs in the experiment were stated in Euro. In each round of the experiment, participants could buy either a rebate token or choose an outside option. In two of the rounds (the *critical* and the *last round*), however, buying a token was only possible with a certain probability, which induced uncertainty about whether a person would reach the rebate or not. Persons were informed about the probabilities of both random events, which could turn out positive (i.e., decision between token or outside option possible) or negative (i.e., round omitted). The critical round took place with a probability of $p_C = .83$. The last round took place with a probability of $p_L = .15$. p_C and p_L were independent and this was common knowledge to all subjects. In order to receive the rebate for the tokens, the person needed to buy tokens in all but one round. Stated differently, the rebate was still granted if one of the random draws turned out negative and the person had bought tokens in all remaining rounds. Hence, the prior probability of reaching the rebate was high ($p_R = p_C + (1-p_C)p_L = .86$). Nevertheless, if the critical round did not take place, this probability was reduced dramatically to $p_R^* = p_L = .15$.

The payoffs and probabilities were set in such a way that if the critical round was omitted (for a subject who bought tokens in every previous round), RCT and CPT would make contrary predictions about staying or quitting the loyalty rebate option: the expected payoff for continuing to buy tokens was lower than that for choosing the outside option. Hence, RCT predicts rational switching to the outside option (see Table 1, second-last row). In contrast, CPT predicts a stickiness effect of rebates and continued buying of rebate token (see Table 1, last row). As the main dependent measure we used buying behavior in the round after the random draw determining whether the critical round takes place or not.

Choice data in the following round was only informative if the critical round was indeed omitted. To avoid data loss for cases in which this was not

the case, we incorporated a strategy method: Prior to the realization of the random event determining whether the critical round would take place or not, subjects committed themselves to decisions in both potential states of the world, i.e., they decided what they would do if the critical round was omitted and what they would do in case it took place. If a round was omitted, it was neither possible to choose the outside option nor to buy a token. After it was randomly determined whether the critical round took place or not, the buying behavior committed to ex ante was implemented automatically. Then participants continued buying in subsequent rounds.

After subjects had gone through the experiment, we elicited risk preferences and the loss aversion parameter λ using the incentivized scales developed by Holt and Laury (2002) and Gächter, Johnson, and Herrmann (2007). The Holt-Laury scale measures risk aversion by letting subjects choose between 10 pairs of lotteries. Each pair contains a low-risk lottery yielding 2 € with probability π and 1.60 € with probability $1-\pi$ and a high-risk lottery yielding 3.85 € and 0.10 € with the same probabilities ($\pi = 0.1, 0.2, \dots, 1$). The number of choices for the low-risk lottery is used as a measure for risk aversion. If, for example, a participant chooses the low risk lottery in 7 (out of the overall 10) decisions, he has a risk-aversion score of 7 (which refers to a specific range of relative risk aversion scores; see Holt & Laury, 2002). The Gächter-Johnson-Herrmann-scale is based on six choices between playing a lottery or rejecting it. Each lottery has a fifty-fifty chance of winning 6 € or losing between 2 and 7 €. For example, if the subject is not willing to play a lottery offering a 50:50 chance of winning 6 € and losing 3 €, it is assumed that the person has a $\lambda > 2$.

4.1.3 Material

In each round, participants had to decide whether to buy a token or to select an outside option while being provided with detailed information (Figure 1). The outside option was to earn 0.44 € per round in which it was chosen. For each token they bought, participants received 1.30 € at the end of the experiment. This value represented the consumption utility of the token. Dependent on condition, the buying price before the rebate was either 1.10 € or 1.25 €. Hence, without a rebate, the payoff of the outside option was much higher than that of the tokens. If the rebate threshold was reached, however, the effective buying price was substantially reduced, so that then the payoff for each token was higher than the outside option. We manipulated the number of rounds in which tokens could be bought from low (10 rounds) to high (15 rounds).

Figure 1: Information display in the decision tasks.

Round 3

You can either purchase a token or choose a direct payment.

Your Balance -2.2 €	Price of token: 1.10 € Price of token, if rebate is reached: 0.75 €	Direct payment: 0.44 €
You have 2 tokens		
Exchange price: 1.30 €/token		
The value of your tokens is 2.60 €		
Rebate: buy at least 14 tokens		

Buy token

Don't buy token

To make the results comparable between conditions, we held the incentives for leaving the rebate scheme after the first random draw, as well as the number of remaining rounds after the critical round, constant across conditions. Consequently, in the low repetition condition the critical round was Round 5, whereas it was Round 10 in the high repetition condition. Furthermore, for all conditions the difference in expected payoffs for remaining in the rebate scheme vs. quitting was held constant (except for small rounding differences).

Table 1: Manipulations and Expected Payoffs.

	Rebate Magnitude			
	Low		High	
	Repetition in Buying			
	Low	High	Low	High
Rebate Magnitude in € (after critical round omitted)	5.06	5.10	9.05	9.01
Repetitions in Buying (rounds)	10	15	10	15
x_1 (price per token w/o rebate)	1.10 €	1.10 €	1.25 €	1.25 €
x_2 (price per token with rebate)	0.56 €	0.75 €	0.25 €	0.61 €
Prospect of staying in rebate (after critical round omitted)	(6.66€; .15; 1.60€)	(7.70€; .15; 2.60€)	(9.45€; .15; 40€)	(9.66€; .15; 65€)
Prospect of quitting rebate option (after critical round omitted)	(2.56€; .15; 3.00€)	(4.00€; .15; 3.56€)	(2.40€; .15; 1.96€)	(2.65€; .15; 2.21€)
EV for staying / quitting in €	2.36/ <u>2.63</u>	3.36/ <u>3.63</u>	1.76/ <u>2.03</u>	2.00/ <u>2.28</u>
CPT V for staying / quitting (see Appendix A)	<u>-6.13</u> /-7.53	<u>-6.17</u> /-7.60	<u>-10.22</u> /-13	<u>-10.18</u> /-12.93

Note. Prospects are given in the format (payoff 1; probability 1; payoff 2).

4.2 Results

Out of 64 subjects, eleven switched back and forth between the rebate and the outside option at least once before the critical round. For these subjects, both RCT and CPT predicted to leave the rebate after the critical round was omitted. Four subjects did not buy a token in round one and kept choosing the outside option consistently until the last round. This behavior of avoiding a rebate scheme can be explained by a strong aversion to risk (see Table 3 below). The remaining 49 subjects (76%), which we will call *target persons* (because they are most informative for testing our hypotheses), entered the rebate scheme and started buying rebate tokens constantly until the critical round.

In line with previous findings (e.g., Holt & Laury, 2002), our participants were mainly risk-averse with an average score of 6.03 ($SD=1.79$), which corresponds to a relative risk aversion of $0.41 < r < 0.68$. Moreover, the Gächter-Johnson-Herrmann-scale showed that the subjects displayed loss aversion to a normal degree ($\lambda = 2.18$, $SD= 0.65$; cf. Appendix A). Four persons answered inconsistently (i.e., did not show a unique switching point, but switched back and forth between accepting and not accepting) and for them no loss-aversion score could be calculated.

4.2.1 Stickiness of Rebates

Our main dependent measure was subjects' choices after the random draw determining whether the critical round took place or not. In case the critical round had taken place, for target persons it yields a higher expected payoff to buy a rebate scheme token than choosing the outside option and CPT makes the same prediction. If the critical round is omitted, however, the outside option will yield a higher expected payoff and it would be rational to switch to the outside option. CPT, by contrast, predicts sticking with the rebate. For both situations (i.e., critical round omitted or not), we coded whether persons chose the option that maximized their expected payoff (expected value / EV), that is, whether they decided in line with RCT or not.

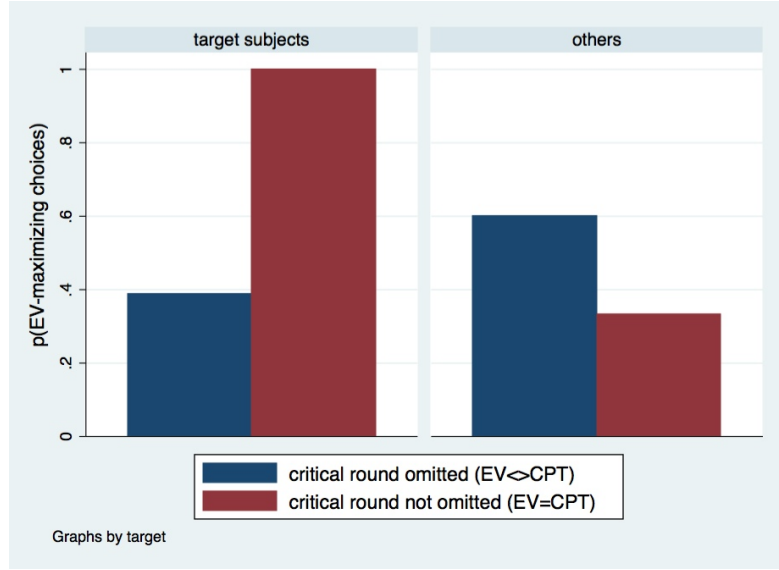
The results indicate a stickiness effect. The proportion of EV-maximizing choices was much higher if the critical round took place as compared to being omitted (Figure 2). In line with the CPT prediction, target persons (Figure 2, left) continued to buy in the rebate even if the critical round was omitted and it was EV-maximizing to quit the rebate. The proportion of EV-maximizers, if the critical round was not omitted and RCT and CPT made the same predictions, is much higher. This difference in proportions turned out significant in an Exact McNemar test, $\chi^2_{df=1} = 30.00, p < .001, N=49$, and the result is robust to including all non-target subjects into the analysis.⁵

Hence, we find strong support for our hypothesis H1, indicating that loyalty rebates are sticky. In accordance with the predictions of CPT, our subjects opted for the choice that yielded greater risk and lower expected payoff.

For the non-target persons (Figure 2, right), it was always rational not to buy the token, which the majority of them also did, regardless of whether the critical round was omitted or not. There was no significant difference in proportions, McNemar $\chi^2_{df=1} = 2.67, p = .21$.

⁵ We include them in two ways into the four-cell test matrix of the McNemar test (the four cells are: always maximize expected value; maximize if critical round is played and not if it is omitted; maximize if critical round is omitted and not if it is played; never maximize). First, we included them by their actual maximizing behavior (for them, maximizing means not buying in the rebate scheme, irrespective of whether the critical round takes place or not). Thereby most, but not necessarily all, of them end up in the always maximize cell, McNemar test, $\chi^2_{df=1} = 18.78, p < .0001, N=64$. Second, we also included them assuming that they had entered the rebate scheme, but counter to our CPT-Hypotheses had always maximized expected payoffs forcing all of them in the always maximize cell, McNemar test scores of $\chi^2_{df=1} = 30.00, p < .001, N=64$.

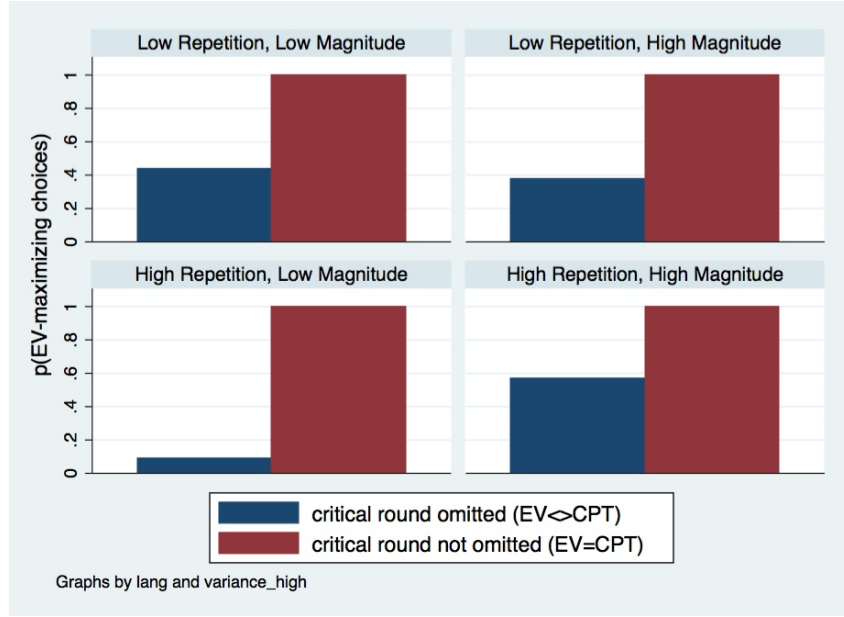
Figure 2: Choices after the critical round.



4.2.2 Effects of Magnitude and Repetition on Stickiness

To test our hypotheses H2 and H3, which state that stickiness increases with magnitude of the rebate, but not with mere repetition in buying, we analyzed choice behavior in the critical round separately for the four conditions, considering the target persons only (Figure 3). The stickiness effect was found in three of four conditions at a conventional and in one condition at a marginal level of significance (low magnitude & low repetition: Exact McNemar $\chi^2_{df=1;N=16} = 9.00$, $p = .004$; high magnitude & low repetition: Exact McNemar $\chi^2_{df=1;N=8} = 5.00$, $p = .062$; low magnitude & high repetition: Exact McNemar $\chi^2_{df=1;N=11} = 10.0$, $p = .002$; high magnitude & high repetition: Exact McNemar $\chi^2_{df=1;N=14} = 8.00$, $p = .008$). All results are robust to including the non-target subjects under the assumption they had maximized EV if they had entered the rebate scheme consistently. Including the non-target subjects according to their actual maximizing behavior leads to insignificant results in the high magnitude & low repetition condition and renders results in the low magnitude & high repetition condition marginally significant (see footnote 5, above).

Figure 3: Choices after the critical round by condition.



For a regression-based analysis, we generated a sticky-buying score. The score was set to 1 if the person bought the token after the critical round was omitted and 0 otherwise. The score hence indicated whether persons performed sticky-buying (1) or not (0). We conducted a logistic regression⁶ with this sticky-buying score as dependent variable and the two condition variables and their interaction as predictors and risk aversion, loss aversion, and gender as further control variables (Table 2).

⁶ We estimated the equation $Y = \beta_0 + \beta_1 X_1 + \dots + \beta_n X_n + \varepsilon$ with logit. A value of $Y=1$ indicated the decision to keep buying in the rebate scheme, $Y=0$ indicated the decision not to buy in the rebate scheme. The variables X_1 to X_7 are the variables and interactions listed in the regression Table 2.

Table 2: Three Logistic Regression Models Predicting Stickiness.

	(1)	(2)	(3)
	Sticky-Buying	Sticky-Buying	Sticky-Buying
Repetition high (0-no, 1-yes; centered)	0.772 (1.01)	1.142 (1.39)	1.141 (1.19)
Magnitude High (0-no, 1-yes; centered)	-1.194 (-1.59)	-1.413+ (-1.77)	-1.955* (-2.06)
IE Repetition*Magn.	-2.850+ (-1.91)	-3.356* (-2.07)	-3.196+ (-1.79)
Gender (0-female, 1-male)		-1.576* (-2.28)	-2.051* (-2.53)
Risk Aversion Score (0-10)			0.280 (0.79)
Loss Aversion (λ)			-0.522 (-0.81)
Constant	0.762* (1.97)	1.578** (2.86)	1.273 (0.58)
Observations	49	49	45
Pseudo R ²	0.108	0.193	0.278

Note. Raw coefficients for a logistic regression on sticky-buying (buying choices after the critical round, i.e., when round 5 or 10 was omitted). Buying indicates stickiness preventing subjects from maximizing expected payoffs. z -statistics in parentheses. Robust standard errors were used. Model 3 includes four observations less due to missing loss-aversion scores. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

We find a significant effect of our magnitude manipulation on stickiness. In contrast to the magnitude hypothesis H2, however, stickiness decreases with increasing magnitude of the rebate (see Figure 3) and H2 has to be rejected. There was no significant effect of repetition on stickiness that allows maintaining the null-hypothesis H3. However, it has to be taken into account that the power of the analysis was relatively low ($1 - \beta = .56$; assuming: odds-

ratio = 2, $p(Y=1 | X=1)_{H_0}=.5$; two-sided test) (Faul, Erdfelder, Lang, & Buchner, 2007).

We also find a (marginally) significant interaction effect of magnitude and repetition, which was not predicted by CPT. High magnitude combined with high repetition frequency decreased the stickiness of a rebate and led to considerably more rational buying behavior.

Additionally, we find a significant gender effect. Female subjects were more inclined to stick to the rebate than male subjects, once they had entered the rebate scheme. Risk aversion and loss aversion had no effect on stickiness (after the person entered the rebate), although CPT predicts that increasing loss aversion should lead to higher stickiness (see Appendix A).

4.2.3 Individual Differences in Entering the Rebate

We were further interested in the question whether there were individual differences in entering the rebate scheme in the first place, dependent on people's risk aversion and loss aversion. One might expect more risk-averse and loss-averse persons to avoid entering rebate schemes altogether in the first place. As mentioned above, the large majority of participants entered the rebate scheme and bought in it until the critical round ($N=49$), but there was also a minority of persons who avoided the rebate altogether and chose the outside option from the beginning ($N=4$). We found higher risk aversion in these rebate avoiders ($M=7.2$, $SE=1.18$) compared to target persons ($M=5.8$, $SE=0.17$), which was marginally significant in a nonparametric test (Mann-Whitney: $p=.07$; one-sided). Similarly, rebate avoiders had higher loss aversion ($M=2.47$, $SE=0.53$) compared to target persons ($M=2.17$, $SE=0.08$), which was also marginally significant (Mann-Whitney: $p=.08$; one-sided).

4.3. Discussion

As expected, we show that loyalty rebates lead to non-rational stickiness, in that consumers do not switch to outside options with higher payoff and lower risk. We did not find support for the magnitude hypothesis and there was no significant effect of repetition on stickiness that allows remaining the repetition null-hypothesis in line with CPT. We also found an unexpected interaction of repetition and magnitude of the rebate.

One of the potential weaknesses of the first experiment is that we show the stickiness effect only as comparison between a situation in which a critical round was omitted and one where it was not. Both situations, however, necessarily differ slightly concerning expected payoff and risk, due to the different number of rounds played so far. Although we find a stickiness effect, we cannot completely rule out further unexpected effects of these differences. Hence,

there remains some doubt whether the investigated rebate scheme was the sole cause of the observed stickiness effect. Therefore, in a second experiment, we compare the decision participants take in the experimental rebate scheme (remain in rebate vs. exit rebate) with a payoff-equivalent choice between two lottery tickets (risky low payoff vs. safe high payoff). In terms of payoffs and risk the lottery tickets are exactly equivalent to the respective two options our participants have in the rebate scheme (remain in rebate = risky low payoff; exit rebate = safe high payoff). If subjects choose the option with high risk and low payoff more often in the rebate scheme than in the lottery ticket choice, as we expect, we can identify the rebate scheme as the cause for the stickiness effect. As the decision in the rebate and the choice between the lottery tickets are equivalent in terms of risk and expected payoff, finding a difference between choices in the rebate scheme and the lotteries tickets could not be explained by RCT. CPT, however would predict this difference due to a reference point shift for rebates.

5. Experiment 2: Sticky Rebates in Direct Comparisons with Gambles

5.1 Method

Participants were mainly students from the University Bonn, again recruited from the MPI Decision Lab subject pool using ORSEE (Greiner, 2004). We assured that individuals took part in only one of the rebate studies reported in this paper. A total of 68 participants (mean age: 24.9, 37 female) took part in the experiment. The study lasted between 60 and 90 minutes and participants received a performance-contingent payoff (range: 0 € to 29.69 €; approximately USD 41.27) in exchange for their participation. Procedure and design were essentially the same as in Experiment 1, except that participants did additionally choose between risky and safe lottery tickets, which were equivalent to the prospects that were involved in their sticky buying decision. Table 1 reveals that the choice between staying in a rebate scheme and leaving the rebate scheme is essentially a choice between two gambles. Take the treatment with a low rebate magnitude and a low repetition of buying rounds (first column of Table 1) as an example. Here staying in the rebate (and consistently buying tokens for the remaining rounds) means choosing a risky gamble with a lower expected payoff yielding 6.66 € with 15% and 1.60 € with 85% probability. Exiting the rebate scheme and never buying a token again means taking a less risky gamble with a higher expected payoff yielding 2.56 € with 15% and 3 € with 85% probability. In Experiment 2, additionally to buying in the rebate scheme participants had to choose one out of two lottery tickets that equaled these payoffs (i.e., lottery ticket 1: 6.66 € with 15% and 1.60 € with 85%; lot-

tery ticket 2: 2.56 € with 15% and 3 € with 85%).

Both the rebate scheme and the lottery tickets were played and paid. This design allows for a direct evaluation of the stickiness effect of rebates because according to CPT for lotteries no shift in reference point should occur.⁷ CPT predicts stickiness only in the rebate task and not in the choice between lottery tickets, whereas RCT would predict equal behavior in both situations. Thus Experiment 2 allows us to identify the rebate scheme as the cause of the stickiness effect because the rebate scheme is present in one task and absent in the other while the choice between the gambles is identical in the two.

5.2 Results

Again, the large majority of participants (i.e., $N = 54$, $p = .79$) entered the rebate scheme and started buying rebate tokens constantly until the critical round. For these target persons, we replicate the stickiness effect, in that the proportion of EV-maximizing choices was much higher if the critical round took place ($p = .96$), as compared to being omitted ($p = .56$), Exact McNemar $\chi^2_{df=1} = 22.00$, $p < .001$, $N=54$. Again, this result is robust to the inclusion of the non-target subjects.⁸ More importantly, we also find the stickiness effect in a direct comparison between persons' behavior in the rebate scheme and in choosing between equivalent lotteries. In equivalent choices mimicking the situation after the critical round was omitted, target persons choose the EV-maximizing, safe outside option in the lotteries ($p = .72$) significantly more often than when buying in the loyalty rebate ($p = .56$; see above), McNemar $\chi^2_{df=1} = 4.26$, $p = .039$, $N= 54$. This effect only proves robust to the inclusion of non-target subjects under the assumption that they would have always maximized if they had entered the rebate, McNemar $\chi^2_{df=1} = 4.26$, $p = .039$, $N= 68$.

The significant decrease of stickiness with magnitude and the interaction of magnitude and repetition could both not be replicated in a logistic regression that was conducted with the same predictors as before (cf. model 3; Table 2). However, the coefficients are in the same direction as observed in Experi-

⁷ Models of expectation based reference points (Kőszegi & Rabin, 2006) may also not predict a reference point shift by the lottery, because they assume that reference points are based on *lagged* expectations. But in our setting subjects chose immediately after they were presented with the choice between the two lotteries tickets.

⁸ McNemar $\chi^2_{df=1} = 16.67$, $p < .001$, $N=68$ if they are included with their actual maximizing behavior; McNemar $\chi^2_{df=1} = 22.00$, $p < .001$, $N=68$, if they are included under the assumption that they had always maximized had they entered the rebate scheme

ment 1 (magnitude: $b = -.45$, $z = -0.76$, $p = 0.45$; IE magnitude x repetition: $b = -.50$, $z = -0.40$, $p = 0.69$). The effect of gender on stickiness did not replicate either ($b = .41$, $z = 0.70$, $p = 0.48$). Also, the differences in risk aversion and loss aversion between rebate avoiders ($n = 4$) and target persons could not be replicated, but were both in the previously observed direction (Mann-Whitney: for risk aversion $p = .48$; for loss aversion $p = .13$; one-sided).

5.3 Discussion

In the second study, we replicate the stickiness effect observed in Experiment 1 and also show that it can be found when directly comparing choices in loyalty rebate schemes with choices between equivalent lottery tickets. The second experiment is also informative concerning the stability of the other observed effects concerning the factors influencing the magnitude of stickiness and whether persons enter rebate schemes or not. The effects of these factors seem to be relatively weak and potentially unstable and should be interpreted with caution.

A classic argument in economics is that biases and irrationality in choice behavior should disappear in repeated market interactions. According to this view, loyalty rebates might be unproblematic because consumers will learn over time that they are detrimental and avoid them further on. We investigated this possibility and the stability of the stickiness effect in a third experiment.

6. *Experiment 3: Stickiness in Repeated Rebate Scenarios*

In the third experiment, participants could decide whether or not to buy in rebate schemes in eight different scenarios. Each of them consisted of ten buying trials. As in the real world, the scenarios differed concerning conditions of the rebates on the goodness of alternative options. To mimic a common situation in reality, we induced uncertainty concerning the alternative option. That is, when making the decision whether or not to enter a loyalty rebate scheme, no information was provided whether an alternative option available later on would be good or bad. Half of the scenarios resembled situations as above, in which switching to an outside option was rational (*switching scenarios*), but stickiness should lead to continued buying. The other half were controls in which the alternative option appearing later on was bad and it was therefore money-maximizing to continue buying in the rebate scheme (*non-switching scenarios*).

6.1 Method

Participants were again mainly students from the University Bonn recruited from the MPI Decision Lab subject pool using ORSEE (Greiner, 2004). A total of 43 participants (mean age: 24.6, 22 female) took part in the third experiment, which lasted about 90 minutes. Participants received a performance-contingent payoff (range: 2.34 € to 19.54 €; approximately USD 3.25 to USD 27.16) in exchange for their participation. The scenarios were manipulated within subjects according to a 2 (switching vs. non-switching scenarios) \times 4 (versions) design. Presentation order was counterbalanced between subjects (i.e., eight different orders determined by Latin squares).

The procedure within each scenario was similar to that in the previous experiments, except that we tried to increase external validity in some respects. For 10 rounds, participants could buy the loyalty rebate option A, but rounds 5 to 10 could all be omitted with a certain probability (e.g., each one of the planned buys could be cancelled). The loyalty rebate was granted if option A had been bought a certain number of times (i.e., 7 or 9 out of 10 times). It was common knowledge that an alternative option B (e.g., competing flight offer) would be available later on - but people had no knowledge concerning the specificities of this option until then. In each round, participants had the option to "do nothing", which was connected with a small cost. In the four switching scenarios, outcomes were constructed so that quitting the rebate scheme and changing to option B was EV-maximizing. Continued buying in the rebate, in contrast indicates stickiness. In the non-switching control scenarios, continued buying was rational. We measured stickiness by the number of buying decisions for the loyalty rebate option A in the round after option B became available. After reading the instructions, all persons worked on a test scenario to assure understanding.

6.2 Results and Discussion

In the majority of scenarios, participants started buying consistently in the rebate scheme ($p = .64$). Analyses were conducted for these cases only. In the switching scenarios, we found a strong stickiness effect. In the round after option B became available, almost two thirds of the persons who had entered the rebate scheme showed non-rational buying behavior and continued buying option A in the rebate scheme ($p = .63$, $SE = .049$).⁹ A similar proportion of continued rational buying option A was observed in the non-switching scenarios ($p = .65$, $SE = .068$). A Wald test revealed that both proportions did not

⁹ This and all following SEs are cluster corrected at the participant level to account for the repeated measurement design (Rogers, 1993).

differ significantly, $F(1, 42) = 0.15, p = 0.70$. This indicates a strong stickiness effect, and indicates that after entering a rebate scheme, consumer decisions seem to be rather uninfluenced by the payoff of the outside option available later on. This irrationality, of course, can lead to substantial financial loss.

Stickiness did not disappear after repeated buying in rebate schemes. Even in the switching scenario presented at the last position, we observed a majority of irrational buying ($p = .60, SE = .16$). Stickiness of loyalty rebates did not reduce with increasing experience, as indicated by a logistic regression predicting irrational buying by presentation order, $b = .06, z = 0.68, p = 0.498$. Hence, in the third experiment, we show the stability of the stickiness effect of loyalty rebates and find no support for the hypothesis that non-rationality reduces with experience.

7. General Discussion

Psychological switching cost induced by loyalty rebates is an important topic for antitrust law and consumer protection law. However, there was a lack of empirical data investigating the effects of such rebates on consumers. Most arguments concerning regulation rested on the assumption of buyers who maximize expected surplus as implied in the standard rational choice theory (RCT). In this paper, we show that loyalty rebates impede rational switching of consumers, thereby inducing a psychological switching cost we call stickiness effect.

We report results from three experiments that investigate loyalty rebates in comprehensive tasks mirroring the particularities of consumer purchases. We used Cumulative Prospect Theory (CPT) to derive predictions concerning buying behavior in rebates.

The core finding of this paper is that, in line with CPT predictions, loyalty rebates induce a stickiness effect in that they impede customers' switching from the rebate product to better (payoff-maximizing) outside options. Experiment 1 establishes the general finding. Experiment 2 demonstrates the effect by comparing choices to continue buying in a rebate scheme to choices between payoff-equivalent lotteries made by the same persons. Finally, Experiment 3 demonstrates the robustness of the stickiness effect by showing that it also holds in somewhat more realistic situations as well as for a medium degree of repeated exposure (i.e., over eight times).

Our experiment was designed to exclude features of a rebate scheme, which would cause or reinforce a "suction effect" predicted by RCT (increasing attractiveness or reduction of risk through successive buying). Rather we designed the decision task so that any form of maximizing expected payoff would predict switching to the outside option (assuming risk neutrality or risk

aversion) or no differences between choices in the rebate scheme as compared to the equivalent lotteries (Exp. 2). Nonetheless we observe considerable stickiness of the rebate in all our three experiments. Therefore we unambiguously showed that target rebates can create psychological switching costs that come on top of those switching costs, which rebates may create according to RCT.

Used strategically, therefore, loyalty rebates have an underestimated potential to foreclose markets inefficiently and to harm consumers. The stickiness effect seems to be strong and led between roughly half and two third of the (target) persons to choose the option with the lower expected value.

7.1 Further findings

As a side aspect, we also investigated the influence of rebate magnitude and buying repetition on the size of the stickiness effect. Overall, the effects of rebate magnitude and buying repetition seem to be a bit unstable. In the first experiment, stickiness significantly decreases with increasing magnitude of the rebate, although CPT predicts the opposite effect. However, the effect could not be replicated in the second study. A null-effect of repetition on stickiness observed in both Experiments 1 and 2 was in line with CPT predictions. Note, however, that the latter cannot be considered clear evidence in favor of the theory because the power of the analysis was relatively low. Furthermore, we found in both studies that people's loss aversion had no effect on stickiness. CPT would have predicted a positive relation. A gender effect that was observed in Experiment 1—as female participants showed a higher stickiness to rebates (even when controlling for differences in risk aversion and loss aversion)—could also not be replicated in a second study. Finally, in the first experiment, we found that individual differences could influence people's willingness to enter rebate schemes in the first place. Rebate avoiders seem to be more risk-averse and loss-averse, compared to persons entering a rebate scheme. We observe a similar tendency in Experiment 2, which was, however, not significant either. Further research is needed to test these effects.

7.2 Implications for the Regulation of Loyalty Rebates

The first and most important implication is that loyalty rebates induce a stickiness effect in consumers. Rebates generate a non-rational psychological switching cost that comes on top of the switching costs considered so far on the basis of RCT. The psychological switching costs increase the potential of loyalty rebates to inflict substantial harm on consumers because consumers will end up with less rent on average than they would end up with in the absence of the rebate scheme. The psychological switching costs may also increase the

potential of loyalty rebates to foreclose consumer markets to entrants: the entrant has to compensate the additional attraction of rebates that we call stickiness by selling his product even more cheaply than he would do otherwise. In case the incumbent has market power, it can (ab)use the psychological switching costs of a rebate scheme to foreclose the market inefficiently to competitors and entrants. The stickiness effect we find therefore provides an argument to treat rebates more restrictively both under antitrust law and under consumer protection law.

We found no support for the Court of Justice of the European Union's opinion that a longer reference period that would induce increased repetitions in buying increases the potential for market foreclosure. There was no effect on stickiness with regard to the instances of buying repetitions.

We think our results can cautiously be extended to professional buyers – bearing in mind the problems of external validity arising when extrapolation results from lab experiments to firm behavior. Our experimental task shares some features with buying in rebate schemes in markets with professional buyers. Therefore our findings provide converging evidence for the results by Beckenkamp and Maier-Rigaud (2006), who explicitly deal with professional buyers. The problems of external validity certainly are smallest when retail units are small and individuals take the relevant decisions. Here our results are likely to apply to professional buyers as well. Indeed, in the *Michelin* cases, the dominant firm Michelin sold to retailers apparently including a significant number of small car repair shops. Here, our findings could well apply. So, all in all, the Commission appears to be right not to have ignored the psychological state of (retailing) buyers in its decision.

7.3 Implications for Modeling Choice Behavior for Loyalty Rebates

The stickiness effect predicted by CPT (with the additional assumption that reference points are shifted to the rebate payoff) was clearly supported by the data. However, we also find that the partially reversed effect of rebate magnitude, the sometimes observed interaction between magnitude and repetition, and the null effect for loss aversion on stickiness cannot be easily explained by CPT. So our experiment cannot identify the perfect behavioral theory to apply to rebate cases in consumer markets. Other avenues for future modeling approaches could include the theory of routines (Betsch et al., 2001) or amending the rational choice framework by introducing some degree of inertia. For policy, however, it is more important to be aware of psychological switching costs that can be used to foreclose markets and harm consumers than finding the “true” model of consumer behavior when facing target rebates.

8. Conclusions

Overall, we conclude that loyalty rebates lead to non-rational buying behavior, amounting to an additional psychological switching cost that can cause substantial financial losses for consumers. This effect increases the potential of loyalty rebates to be used as a tool to foreclose markets and provides an argument for a more restrictive position towards loyalty rebates under consumer protection law. Previous arguments and rulings concerning the regulation of loyalty rebates under antitrust law both in the EU and in the US were mainly based on the assumption of rational buying. Stickiness effects add to these existing problems. Therefore the potential danger of loyalty rebates has been underestimated. The demonstrated stickiness effect backs the role psychological effects already play in European antitrust law today. It generally supports the greater scrutiny loyalty rebates have recently been subject to both in the EU and the US.

9. References

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10. Appendices

10.1 Appendix A

Let x_1 and x_2 be the possible monetary outcomes (payoffs) for a prospect and assume p_1 and $1-p_1$ to be the probabilities that the respective outcomes realize. The expected value for this prospect is given by:

$$EV = p_1 x_1 + (1 - p_1) x_2 \quad (1)$$

and, according to rational choice theory, persons should be indifferent between this prospect and any equivalent cash amount c :

$$c = EV . \quad (2)$$

According to CPT, the value V of a prospect with outcomes $x_1 \leq \dots \leq x_k \leq 0 \leq x_{k+1} \leq \dots \leq x_n$ is given by:

$$V = \sum_{i=1}^k \pi_i^- v(x_i) + \sum_{j=k+1}^n \pi_j^+ v(x_j), \quad (3)$$

with v as continuous and strictly increasing *utility function* satisfying $v(0) = 0$, and π^+ and π^- as *decision weights*, for gains and losses respectively. Decision weights result from rank-dependent transformation of the outcome probabilities, considering gains and losses separately. This means that the same probability can result in different decision weights, dependent on whether it belongs to a high or a low outcome. Decision weights are defined by:

$$\begin{aligned} \pi_1^- &= w^-(p_1) \\ \pi_n^+ &= w^+(p_n) \\ \pi_i^- &= w^-(p_1 + \dots + p_i) - w^-(p_1 + \dots + p_{i-1}) \quad \text{for } 1 < i \leq k \\ \pi_j^+ &= w^+(p_j + \dots + p_n) - w^+(p_{j+1} + \dots + p_n) \quad \text{for } k < j < n \end{aligned} \quad (4)$$

with w^+ and w^- being the probability weighting function for gains and losses, respectively. Hence, the lowest negative outcome and the highest positive outcome are transformed using the respective transformation functions described in the next section. The weights for probabilities of losses (i.e., $i < k$) conceptually represent the marginal contribution of the respective probability to the total probability of worse outcomes and the weights for probabilities of gains (i.e., $j > k$) represent the marginal contribution of the respective probability to better outcomes.

For CPT, several functional forms of v , and w^+/w^- have been suggested (see Stott, 2006, for an overview). We use the classic one-parameter implementation of the value function and the weighting function suggested by Tversky and Kahneman (1992):

$$\begin{aligned} v(x) &= x^\alpha & \text{if } x \geq 0 \\ v(x) &= -\lambda(-x)^\beta & \text{if } x < 0 \end{aligned} \quad (5)$$

and

$$\begin{aligned}
w^+(p) &= \frac{p^\gamma}{(p^\gamma + (1-p)^\gamma)^{1/\gamma}} \text{ if } x \geq 0 \\
w^-(p) &= \frac{p^\delta}{(p^\delta + (1-p)^\delta)^{1/\delta}} \text{ if } x < 0
\end{aligned} \tag{6}$$

The risk-aversion parameters α and β capture the curvature of the s-shaped value function. The parameters γ and δ capture the inverted s-shape of the weighting function, in the domains of gains and losses, respectively. The loss-aversion parameter λ induces the increased steepness of the value function in the domain of losses. Tversky and Kahneman (1992) suggested the following parameters: $\alpha = \beta = .88$, $\gamma = .69$, $\delta = .61$, $\lambda = 2.25$.

Let us assume that x_2 is adopted as a reference point and payoffs are perceived as differences from x_2 . Consequently, x_2 has a utility of zero and x_1 has a negative (or zero) utility and the value V_p of the prospect is given by:

$$V_p = v(x_1 - x_2)\pi_1^- = -\lambda(-(x_1 - x_2))^\beta w^-(p_1) . \tag{7}$$

Choosing the cash equivalent c of the prospect (equation 2) will be considered a sure loss because it will also always be smaller than x_2 . According to core predictions of prospect theory, people will prefer a risky option over a sure loss with equal expected value which follows from the fact that the utility function v is convex for losses. Formally, this results in the following value of the cash equivalent V_c :

$$V_c = v(c - x_2) = -\lambda(-(c - x_2))^\beta \tag{8}$$

And when substituting c by equations 1 and 2:

$$V_c = -\lambda(-(x_1 p_1 + x_2(1 - p_1) - x_2))^\beta = -\lambda(-(x_1 - x_2))^\beta p_1^\beta . \tag{9}$$

The difference between V_p and V_c is given by:

$$V_p - V_c = -\lambda(-(x_1 - x_2))^\beta w^-(p_1) - \lambda(-(x_1 - x_2))^\beta p_1^\beta , \tag{10}$$

which can also be written as:

$$V - V_c = \left(-\lambda(-(x_1 - x_2))^\beta \right) \left(w^-(p_1) - p_1^\beta \right) . \tag{11}$$

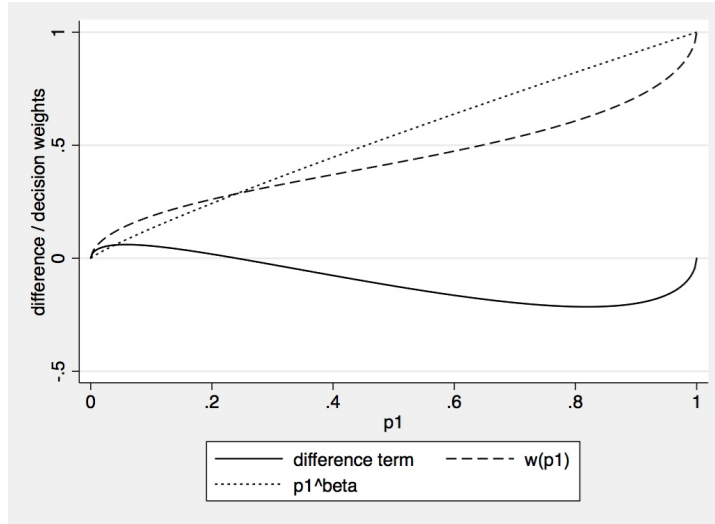
The first term of equation 11 will be negative for all $x_2 > x_1$ and its magnitude increases with increasing difference between x_1 and x_2 . Taking into account the values for parameters $\beta = .88$ and $\delta = .61$, mentioned above, the second term is negative for all probabilities $p_1 > .24$; which is where the functions $w(p_1)$ and p_1^β intersect (Figure A1). Hence, for all $p_1 > .24$ the value of the prospect is higher than its cash equivalent and (everything else being equal) the difference increases with increasing difference between x_2 and x_1 .

Choices between the prospect and the cash equivalent will most likely not be deterministic. It is more likely that they follow a probabilistic function such as a logistic-choice function in which the probability for choosing one

option over the other increases with its advantage in V_p (i.e., the absolute difference between $V_p - V_d$).

Taking an individual differences perspective and considering only prospects with sufficiently likely lowest outcomes to prefer the prospect over the cash equivalent, the degree to which the risky prospects are preferred over the cash equivalent should increase with increasing loss aversion λ . Increasing risk aversion β increases the magnitude of the first term, but decreases the magnitude of the second term in equation 11, and the overall effect is therefore complex.

Figure A1: Difference in decision weights according to the second term in equation 11 as a function of probability of the lower outcome for the domain of losses.



Relation to Rebates

If one accepts that rebates lead to adopting the payoff of reaching the rebate (i.e., x_2 = the maximal payoff) as reference point, then, according to CPT, rebates should induce persons to continue buying in the loyalty rebate scheme, even if an outside option has the higher expected value. This, however, should only hold when considering rebates with sufficiently large probability of failing to reach the rebate ($p_1 = 1 - p_R > .24$). Hence, in our paradigm, CPT predicts entering the rebate because $1 - p_R = .14$ and stickiness to the rebate after the critical round was omitted because $1 - p_R^* = .85$. The probability to stick to the rebate (i.e., staying in the rebate although it does not maximize expected value) should increase with increasing difference between V_p and

V_c which is a monotonously increasing function of the difference between the high and the low overall payoff that can be reached with the rebate option. It should be independent of the repetitions of buying when holding the difference in payoffs constant. From an individual-difference perspective, stickiness should increase with increasing loss aversion and might be influenced in a complex way by risk aversion.

10.2 Appendix B: Instructions

In the first part of the experiment, you can make a buying decision in each round. There are 10 buying decisions in total. The decision is whether or not to purchase a token. You will receive information about the repeated decision in the form presented below. [Figure omitted]

Please read this information carefully now and during the experiment. In this situation, each token costs 1.10 € and has an exchange value of 1.30 €; that is, at the end of the experiment, you will be credited 1.30 € for each token that you purchased during the experiment. In each round, you may purchase one token for the price of 1.10 €. Alternatively, you can also decide not to purchase a token. For each round in which you decide not to buy a token, you will be credited 0.44 € immediately as direct payment.

At the end of the experiment, you will be granted a rebate of 49% on all purchased tokens, provided that you have purchased at least 9 tokens during the first part of the experiment. In this case, the purchase price that you spent on the tokens will be reduced by 49% to 0.56 €.

[Figure omitted] Rounds 5 and 10 are omitted with certain probabilities. If a round is omitted, you can neither buy a token nor choose the direct payoff. Round 5 is omitted with a probability of 17% and Round 10 is omitted with a probability of 85%. [Figure omitted] Dependent on whether Round 5 is omitted or not, the probability for your being able to play 9 rounds varies. Because the experiment can take different directions, depending on whether Round 5 is omitted, after Round 4 you will be asked how you will decide in Round 5 if it takes place, and how you will decide in Round 6 if Round 5 takes place. After these decisions, the computer will determine whether or not Round 5 takes place and you will make the decisions you indicated. If you decide not to buy in Round 5 and the round is played, the computer will only allow you to make this decision. If Round 5 is omitted, the computer will, for Round 6 also, only allow you to make the decision you indicated. In the following rounds, similar to Rounds 1 to 4, you can again choose between buying the token and the direct payment.

Your payment for the first part is calculated as follows:

- If the rebate is granted:
Rounds in which tokens were bought x (Exchange value of the tokens – Price of the tokens) + Rounds in which direct payment was chosen x Value of the direct payment + Price of the tokens x Rounds in which tokens were bought x Rebate
- If the rebate is not granted:
Rounds in which tokens were bought x (Exchange value of the tokens – Price of the tokens) + Rounds in which direct payment was chosen x Value of the direct payment

[Instructions for measures of risk aversion and loss aversion and example calculations are omitted.]

10.3. Appendix C: Questionnaire, Experiment 1 and 2.

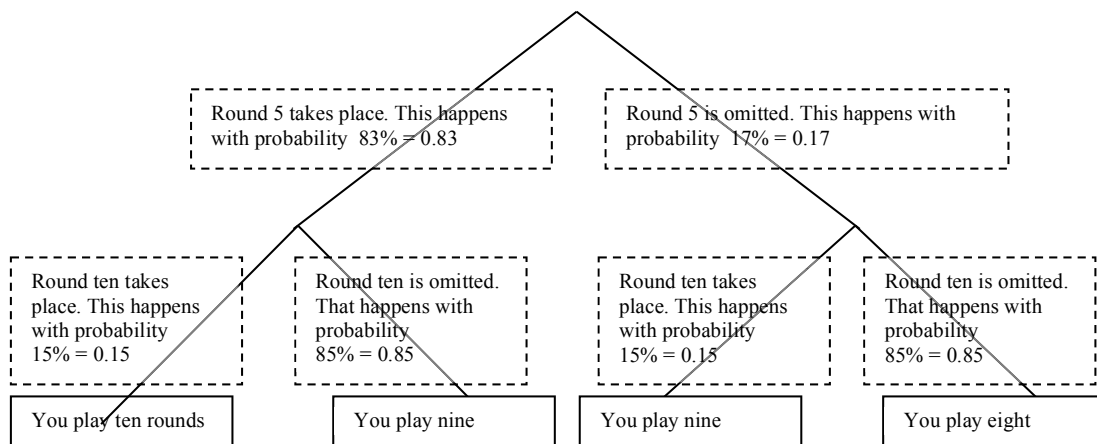
[Subjects were only allowed to proceed with the experiment once all questions were answered correctly.]

Please copy all the important parameters into the following list:

Price of tokens	_____
Value of tokens	_____
Minimum number of tokens bought, required to attain the rebate.	_____
Rebate	_____
Value of the outside option	_____
Probability that round 5 will be omitted	17 % = 0.17 (please calculate in decimal fractions in this questionnaire).
Probability that round 10 will be omitted	_____
Exercises:	

- 1) How much do you earn in the first part of the experiment in case you buy 10 tokens?
 - 2) How much do you earn in the first part of the experiment in case you buy 9 tokens?
 - 3) How much do you earn in the first part of the experiment in case you buy 9 tokens and choose the outside option once?
 - 4) How much do you earn in the first part of the experiment in case you buy 5 tokens and choose the outside option five times?
 - 5) In the first part of the experiment, will you be able to buy 10 tokens for sure?
 - 6) In the first part of the experiment, is it certain that you will attain the rebate if you want to? If yes, why? If no, what does getting the rebate depend on?
 - 7) How many decisions will you at least take in the first part of the experiment? How many at most?
 - 8) How much will you earn in case you choose the outside option nine times?
- [questions omitted that elicited the willingness to pay for not participating in the experiment that are not relevant for the current study]*
- 9) At the beginning of the experiment, what is the probability that you will play ten rounds in the first part of the experiment?
 - 10) At the beginning of the experiment, what is the probability that you will play nine rounds in the first part of the experiment?
 - 11) At the beginning of the experiment, what is the probability that you will play eight rounds in the first part of the experiment?

If you find it difficult to solve exercises 12-14, consider the following tree diagram, which may help you to see what can happen in the game through omitted rounds.



III. The Short Arm of Guilt: Does it only hit who is close?

1. Motivation

A couple of years ago, the motivational power of people's desire to avoid guilt reached the economic literature and was called guilt aversion. Among other things, guilt aversion was used to explain why people keep their promises in an investment game (trust game) and it was modeled as a preference to choose actions conforming with another person's expectation (Battigalli & Dufwenberg, 2007; Charness & Dufwenberg, 2006). Surprisingly, the economic model of guilt aversion has not granted a lot of attention to the relevance of social distance. Why should I feel guilty with regard to someone I do not care about? Or why should I feel guilty about letting someone down if I was not responsible for his well-being? Psychologists treat the sensitivity of people to others' expectations as inherently context-dependent. And this context is shaped by relationships. In fact, Charness and Dufwenberg (2006) motivate their research on guilt by citing articles from psychology which deals with the impact of relationships on guilt and postulates a "communal relationship" as precondition to experiencing guilt vis-à-vis a person (as opposed to a mere exchange relationship), for instance Baumeister, Stillwell, & Heatherton (1994). Other psychological theories postulate the expansion of one's own self to that of a relationship partner as a prerequisite to experience guilt (Aron, Aron, & Norman, 2003; Aron, Aron, Tudor, & Nelson, 1991).

I assess the impact of social closeness on guilt aversion in a dictator game by systematically varying shared social identity using a minimal group paradigm. If even group identity induced in the lab changes how second-order beliefs (i.e., the sender's expectations over the receiver's expectations) induce actions, the effect is likely to be stronger by magnitudes in the field.

In each session, I allocate half of the participants to one and the other half to a second group according to their respective stated preference for one of two modern painters. I further reinforce their respective group identity (Tajfel & Turner, 1979) by letting the two groups compete for a prize in a real-effort task and subsequently measure to what extent participants identify with their group. Then I randomly match participants in pairs, half of them with a partner from their own group (ingroup treatment) and the other half with a partner from the other group (outgroup treatment). These pairs play the following variant of a dictator game. From receivers I elicit their belief about what they expect the sender to send to a receiver in their situation (from the same group or from the

other group, respectively). Then, by means of the strategy method, I elicit what share of a pie of 100 “Talers” a sender wishes to send to the receiver conditional on 11 possible second-order beliefs. I look at realistic second-order beliefs in isolation first. I speak of realistic second-order beliefs if the sender expects the receiver to expect the sender to send half of the pie at most because amounts sent that exceed half of the sender’s endowment are extremely rare (Engel, 2011). In the realm of realistic beliefs, I find that the influence of second-order beliefs on the amount sent in a dictator game is stronger if the receiver shares the sender’s group identity. For the analysis of exaggerated second-order beliefs, the reader is referred to Appendix A. I speak of exaggerated second-order beliefs if the sender expects the receiver to expect the sender to send more than half of the pie.

In both treatments, about half of the senders remain unaffected by second-order beliefs. If senders and receivers are from the same group, unaffected senders are characterized by lower degrees of group identification. This is not true for the treatment where senders and receivers are from different groups.

My findings are relevant for certain questions that have been studied for a long time, yet remain unresolved. These findings concern promises, ingroup favoritism and the theory of guilt aversion.

The results of this experiment suggest that people may keep unenforceable promises because the promise establishes a closer relationship between the parties. My amendment of guilt aversion reconciles the contradicting results of Charness & Dufwenberg (2006; 2010), on the one hand, and those of Vanberg (2008), on the other. Charness & Dufwenberg (2006; 2010) explained why people keep a promise to a stranger by the fact that promisors know that the promise raises the promisee’s expectations. Vanberg (2008) found that promisors keep their own promise, but did not respond to second-order expectations induced by a thirdparty’s promise. If guilt aversion depends on social integration, Vanberg’s result can be explained by a lack of social integration between a promisor with a third-party promisee. That lack of social integration in turn leads to a lack of influence of second order beliefs on action.

Güth, Ploner & Regner as well as Ockenfels & Werner, theorise that participants favour fellow group members because senders know that receivers expect more in ingroup interactions than in outgroup interactions. According to my findings, this explanation is at least incomplete. Explanations along the lines of Güth et al. and Ockenfels & Werner should take into account that second-order beliefs also matter more in ingroup interactions than they do in outgroup interactions. If second-order beliefs have a stronger effect ingroup than outgroup one would predict ingroup favoritism to arise even if receiver expectations are held constant. The only prerequisite would be that group identity is strong enough.

My main finding that shared identity induces the influence of second-order beliefs on action explains why guilt aversion has been rejected in anonymous experiments (i.e., Ellingsen, Johannesson, Tjøtta, & Torsvik, 2010), while it has been confirmed in experiments that allow for some form of relationship between participants (Charness & Dufwenberg 2006, Reuben et al. 2009). This finding structures the hitherto inconclusive literature on guilt aversion (for a review, see below). My finding also moderates conclusions that guilt aversion has been rejected in its entirety and specifies the realm of application of guilt aversion to social interaction across a small social distance.

2. Literature

This paper will investigate whether a model of guilt aversion, which is sensitive to the degree of social intergration between agents, leads to better predictions than the existing formulations of the theory. The blind spot of guilt aversion with regard to relationships seems to have led to inappropriate test beds when testing the theory. The beginnings of the lab career of guilt aversion were promising (Charness & Dufwenberg, 2006). However, further tests of the theory of guilt aversion in the lab led to mixed results: Vanberg (2008) conducts a dictator game experiment where he claims to separate the effect of the mere promise and that of second-order beliefs. He reshuffles half of the sender-receiver pairs after the promise, leaving only the receivers uninformed about whether their pair has been reshuffled. This leaves receivers' beliefs constant across treatments. Treatments only vary by whether the sender is bound by a promise to the receiver or not. Vanberg finds an effect of the promise, although second-order beliefs are constant over treatments. Although in his appendix he presents some evidence that second-order beliefs correlate with action, he cannot show a causal effect of second-order beliefs on action. He concludes that second-order beliefs cannot explain the effect of promises in trust games. Reuben et al. (2009) conduct an experiment in which they elicit investors' beliefs in a trust game and report them to trustees. They do find evidence in favor of guilt aversion. Ellingsen et al. (2010) conduct a series of dictator-game and investment-game experiments where receivers report their beliefs on the amount sent to the experimenter and the experimenter reports these beliefs to the senders, inducing second-order beliefs. Ellingsen and coauthors find evidence that second-order beliefs do not determine action, but that actions induce second-order beliefs. Lately even prominent promoters of guilt aversion, Charness and Dufwenberg (2010), merely found "limited support" for guilt aversion. And finally, in a trust game with an investor, a trustee, and two inactive players, Bellemare et al. (2011) found trustees to have a positive willingness to pay to avoid guilt vis-à-vis the investor only. At first glance, the literature could lead the reader to believe that the correlation between second-order be-

liefs and actions is an instable phenomenon that tends to be revealed as a confound – either with a preference to keep a promise (Vanberg, 2008) or with a (false) consensus effect (Ellingsen et al., 2010).

However, re-analyzing the experiments just mentioned with regard to the intensity of the relationship between subjects and the findings of guilt aversion, a correlation seems to emerge. Charness and Dufwenberg (2006) use pre-play communication by means of a one-page free text letter in a classroom experiment where subjects can see each other. This protocol is apt to make participants feel closer to each other. They find second-order beliefs to correlate with actions. Vanberg (2008) claims to disentangle the effect of the promise from that of expectations by rematching half of the participants randomly after communication in an anonymous, computerized dictator game. However, this protocol does not merely destroy the promise of randomly rematched dictators; rather, it also destroys the social relationship participants may have built through the promise. Vanberg concludes that people have a preference for keeping a promise, independently of second-order beliefs. Reuben et al. (2009) use 56 subjects in one session, all of whom were MBA Students at the Kellogg Business School. This school does not have more than 650 students in total. Given that MBA programs are meant to establish close networks among their students, it is not unlikely that there was some *esprit de corps* connecting the subjects in this setting. Accordingly, Reuben et al. (2009) find evidence of guilt aversion. Ellingsen et al. (2010) use an anonymous double blind protocol. The only contact amongst participants is that beliefs elicited from the receiver are reported to senders – a procedure specifically meant to exclude any social integration of participants. Reuben et al. do not find any correlation between second-order beliefs and action. Charness and Dufwenberg (2010) adopt a protocol enabling senders either to make a promise to receivers by sending a pre-formulated sheet of paper or not to make a promise by sending an empty sheet of paper. This procedure does not allow for any personalized contact between participants, but involves a promise. The anonymity of this procedure may be the reason why Charness and Dufwenberg (2010) do not find clear support for guilt aversion in their experiment. Bellemare et al. (2011) only find guilt aversion of the trustees vis-à-vis the investors. But the trustees have no willingness to pay to avoid guilt vis-à-vis the inactive players. In fact, although the setup of the experiment is anonymous in that subjects participated online from their homes, the contrast between the investor who actually does act with effect on the trustee and the inactive players who does not may have induced the trustees to feel closer to the investors.

Of course, this juxtaposition of six experiments is far from being conclusive evidence for guilt aversion only to play out if agents are socially integrated. But it may be a hint. And given that the psychological theories from which the

economic theory of guilt aversion was originally derived accord a prominent role to relationship, the hint merits to be taken seriously. A serious test of this hint seems all the more warranted as some parts of the literature fit the pattern found in the six cited papers less well. Dufwenberg and Gneezy (2000) find that the trustees' second-order beliefs correlate with actions in a trust game, although they apply a double blind and thus very anonymous procedure.¹⁰

There is a large literature showing that decreasing social distance (Charness & Gneezy, 2008; Frey & Bohnet, 1999; Hoffman, McCabe, & Smith, 1996; Leider, Möbius, Rosenblat, & Do, 2009, 2010; Rankin, 2006), increasing social integration (Brañas-Garza et al., 2010), or inducing a common group identity (Chen & Li, 2009; Dawes, Van de Kragt, & Orbell, 1988) between participants leads social preferences to play out more strongly in dictator games. But none of the cited studies treats belief-dependent preferences.

The literature studying the relevance of second-order beliefs has not produced an answer either to the question whether guilt aversion requires some form of social closeness. Rankin (2006) studies whether receivers' demands in a dictator game have different effects if receivers and senders communicate face to face or anonymously. I study the effect of second-order beliefs and not of demands. In contrast to outright demands, the beliefs I work with do not have any normative or imperative appeal. Bicchieri and Chavez (2010) study the impact of first- and second-order beliefs on transfers in an ultimatum game. In the ultimatum game, however, second-order beliefs are strategically relevant. Guilt aversion claims an influence of strategically irrelevant beliefs. Therefore the study does not provide evidence on whether people have a preference to act in accordance with second-order beliefs. Recently the idea arose that in-group favoritism is caused by changes in second-order beliefs. Güth et al. (2009) as well as Ockenfels and Werner (in press) hypothesize that senders treat ingroup receivers preferentially because they know that ingroup receivers expect them to send more. Güth et al. do not find clear support for this hypothesis. Ockenfels and Werner find that indeed senders treat ingroup receivers better if the latter know that they share the sender's group identity. This effect is attenuated if the receiver does not know of the sender's group identity. I am not interested in the effect of changing levels of second-order beliefs on action. I ask for the effect of shared group identity on the capacity of second-order beliefs to induce action.

¹⁰ They neither control for the false consensus effect, nor can they claim causality of second-order beliefs. So causality could also run from action to beliefs or there could be a confounding variable.

3. Experiment

I run an experiment with two treatments, an ingroup treatment and an out-group treatment. The on-screen experiment is programmed in z-tree (Fischbacher, 2007). Group membership is induced, conditional on the subjects' preferences for paintings (Chen & Li, 2009), and reinforced by letting groups compete in a real-effort task (Rockenbach, Böhm, & Weiss, 2013). Each of the 15 sessions conducted in the Bonn EconLab comprises 16 participants recruited via ORSEE (Greiner, 2004). Each subject receives a show-up fee of 4 €. In the experiment, the subjects play for the experimental currency "Talers". Participants are paid in Euros. 1 Taler converts to 0.11 €. The experiment proceeds as follows.

All participants are seated in front of a computer terminal, separated by cubicles. They are first asked to compare paintings by Klee and Kandinsky (group segregation stage); then, they compete in a real-effort task (group reinforcement stage) and fill out a pre-experiment questionnaire (questionnaire stage); and finally, they play a dictator game (game stage).

3.1 Group Segregation Stage

In the group segregation stage, each participant is assigned to one of two groups according to his/her preference for one of two painters – Klee and Kandinsky. The procedure is adapted from Chen and Li (2009): For five pairs of paintings by Klee and Kandinsky (the same paintings as in Chen and Li (2009)), subjects are asked to state how much they prefer one painting to another. To answer this question, participants use a slider bar (labeled in three steps [L=left, R=right]: I strongly prefer L, I like both paintings equally, I strongly prefer R). The position of the slider bar is translated into a distribution of 10 points between the two paintings (I strongly prefer L = 10 points to L, I like both paintings equally = 5 to L and 5 to R, etc.). Then, for each participant, the points allocated to Klee are summed up. The same is done for the points allocated to Kandinsky. Subsequently the computer labels that half of participants who allocated the highest amounts of points to Klee the "Klee group" and labels the other half the "Kandinsky group". Participants then are informed about their group membership.¹¹

¹¹ Ties were resolved by the order of the randomly assigned subject ID. Subjects could allocate fractions of points, so that virtually an infinite number of possible sums of points for Klee paintings were possible. The slider bar did not have a scale beyond the three labels mentioned in the text. Therefore it was virtually impossible to set it to precise integers (other than 10.0; 5.5; 0.10). Be-

3.2 Group Reinforcement Stage

In the group reinforcement stage, the two groups compete against each other in a real-effort task to intensify the perception of belonging to a group by experiencing interdependence and a common fate. The task subjects compete in is the following (Rockenbach et al., 2013). Participants receive a 15-page text. Then, on their screens, I ask them for letters in the text that I define by page, line, word, and position. Participants have four minutes to identify as many letters as they can. The group that jointly accumulates the larger number of correct answers wins. Each participant of the winning group receives 26 Talers. If the groups tie, all participants receive 13 Talers. Participants do not receive any feedback on the between-group competition until the very end of the experiment, which is why independence of observations is preserved.

3.3 Questionnaire Stage

The computer randomly assigns half of each group to the role A (sender) and the other half of each group to the role B (receiver). Then the computer randomly pairs each sender with a receiver. Half of the senders will be paired with a receiver from their own group (ingroup treatment) and half of the senders will be paired with a receiver from the other group (outgroup treatment).

In the questionnaire stage, the senders (A) are not informed about their role. They answer a questionnaire on how much they identify with their group (Doosje et al. 1995; see appendix B.3.2b. for questions).

The receivers (B) are informed about their role and the group membership of the participant in role A they have been paired with. On their screens they are informed that in the subsequent stage they will be paired with a sender, which group this sender belongs to, and that the sender can freely split 100 Talers between himself and the receiver. They are then asked to predict the average amount a person in their situation – i.e., a receiver paired with a sender from the same [the other] group – would receive in the experiment. The responder can enter any guess, which can be expressed in a full amount between 0 and 100 Talers. It is announced that each subject who predicts an average amount (which is no more than 1 Taler off the actual average amount received by receivers in their situation during the session) will receive an extra payment of 125 Talers = 13.75 € (see Appendix B.3.2.a for details). After all participants have completed their respective questionnaires, the questionnaire stage ends.

cause ties thus were extremely unlikely, the tie-breaking rule was not included in the instructions. However, a tie occurred twice.

Of 120 senders, ten ended up in the Kandinsky group although they had awarded more points to Klee. Two who had given exactly the same amount to Klee as they gave to Kandinsky ended up in the Kandinsky group.

3.4 Game Stage

Subjects play a sender-receiver game (dictator game). On the first screen, all participants are informed about their role. Also, all participants are reminded about their own group membership and informed about the group membership of the participant they have been paired with. Senders receive a pie of 100 Talers. They can send any share to their respective receiver, which can be expressed in full Talers. The amount sent is elicited by means of a strategy method. Senders are asked what they would like to send, conditional on their respective receiver's belief. They express the amount they wish to send for the receiver's beliefs of 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, and 100 Talers. It is explained to them that the computer will activate the choice closest to the receiver's actual stated belief.¹² After the senders have filled in the strategy vector, a screen will reveal the true stated belief of the sender, and the computer will put into effect the allocation for the case closest to that belief.

The use of the strategy method described does not confound the treatment effect with an experimenter demand effect, because the experimenter's "demand" to condition the amounts sent on second-order beliefs remains constant over treatments.

Apart from my treatment manipulation (ingroup vs. outgroup) and computerization, the design described amends that of the paper that mainly motivated this work (Ellingsen et al., 2010) merely in that I use the strategy method to let senders condition their amounts sent on different second-order beliefs instead of just one. Generally, this method of letting senders condition their amounts sent on receivers' previously stated beliefs seems to be standard in the literature on guilt aversion (Bellemare, Sebald, & Suetens, 2013; Ellingsen et al., 2010; Reuben et al., 2009).

Finally, in a posttest, I measured perceived closeness between the sender and the receiver, using a one-item test by Aron et al. (2003), and I ask participants for some demographic data, such as gender, age, and occupation.

4. Theory and Hypothesis

To illustrate my theoretical point, I use the simplest formulation of guilt aversion. It can be found in Charness and Dufwenberg (2006). I will extend their model to include social distance as a driving force of guilt aversion. I will justify my amendment with the help of psychological and economic theory. Finally I will derive and specify the hypothesis of my experiment. For conjectures

¹² Receivers' stated beliefs are rounded to 0 or the closest multiple of 10 according to general rounding conventions.

about what could plausibly be expected to happen in the realm of exaggerated beliefs, see Appendix A.

4.1 Theory

For a simple two-strategy dictator game, Charness and Dufwenberg (2006) propose a simple definition of the utility u_s of the outcome given the sender S chooses A (the selfish option) over B (the generous option):

$$u_s = \pi_A - \gamma_s \cdot \pi_B \cdot \tau_s$$

The sender's utility is increased by his money payoff of choice A, but it is reduced by a guilt term. The sender's money payoff of option B is denoted by π_B . The sender's sensitivity to guilt is $\gamma_s \in [0,1]$. Finally, $\tau_s \in [0,1]$ denotes the sender's second-order belief about how likely he thinks the receiver believes the sender to choose the generous option. The guilt term increases in all of these variables.

This utility function claims second-order beliefs influence the attractiveness of the selfish choice. Senders therefore should always respond to information from which they derive their second-order beliefs by being more or less prone to act more selfishly. We have not observed this consistently in experiments. Ellingsen et al. (2010), in particular, conducted a high-powered experiment that could not show any reactions of senders to second-order beliefs. The hunch derived from the literature above was that possibly people only condition their action on second-order beliefs if they interact with somebody to whom they feel close. Accordingly, I propose to let the guilt term in the utility function also depend on a measure of social closeness. Multiplying the guilt aversion part of the utility with $\alpha_s \geq 0$, where α_s is the sender's appreciation of how close his relationship with the receiver is, would express this dependence. The larger α_s , the closer the relationship. An α_s that is equal to 1 expresses guilt aversion as Charness and Dufwenberg (2006) defined it. α_s equal to 0 would indicate a social distance too large to trigger any feeling of guilt.

$$u_s = \pi_A - \gamma_s \cdot \pi_B \cdot \tau_s \cdot \alpha_s$$

The justification of this extension lies in a recombination of the psychological theory of how closeness translates into empathy with the theory of reference-dependent preferences. In economics, other-regarding preferences à la Charness and Rabin (2002) or Fehr and Schmidt (1999) have been modeled to include the other's payoffs into the self's utility function. Guilt aversion goes a step further including the self's beliefs about the other's expectations into the

utility function. Why do beliefs about expectations matter at all? Guilt aversion does not take a clear position on this question. One answer is that beliefs are important because we know the other's expectations matter for the other's utility. According to reference-dependent utility, expectations shift reference points (Abeler, Falk, Götte, & Huffman, 2009; Köszegi & Rabin, 2006). And outcomes below the reference point are coded as losses, while those above are coded as gains. Losses loom larger than gains so that any outcome short of the expectation would have a strong negative impact on utility. Writing the impact of expectations on the other's utility into the self's utility would mean including second-order expectations in the self's utility function. In fact, the self's utility would include parts of the other's (reference-dependent) utility (instead of just plugging the other's payoff into the self's utility function). Economists seem reluctant to integrate the other's utility (as opposed to the other's payoffs) into the self's utility function because this would yield complex interdependence of agents' utility, rendering these utility functions difficult to use. Psychology has been bolder and has developed theories that are equivalent to the self integrating the other's utility into her utility functions. Psychologists frame the integration of the other's utility function into the self's as "self expansion", meaning the extension of one's own self to encompass other individuals' selves (Aron et al., 2003, 1991; Hewstone, Stroebe, & Jonas, 2008). In particular "participants in a close relationship include each other into their psychological selves" (Aron et al., 2003). Other authors describe that same thing, saying that "oneness" increases among ingroup members (Brewer, 2007). In a slightly different approach, focused on norms rather than utility, Baumeister et al. (1994) claim that guilt only arises due to a violation of norms induced by a "communal relationship". "Communal relationships are defined by the existence of implicit rules that the individuals must be concerned about each other's welfare (...). As a result, communal relationship partners do things simply to benefit each other without expecting equal or immediate benefits in return" (Baumeister et al., 1994). Psychological theory predicts that with a sufficiently close relationship between the self and the other comes the self's concern for the other's welfare. Accordingly, in my experiment, senders of the ingroup treatment would be expected to experience the utility they cause in receivers of their own group as their own utility to some degree. If the receivers' utility depends on their expectations, as reference-dependent utility suggests, this means that, according to self-extension theory, senders should behave in line with receivers' expectations to a larger extent if receivers are from their own group than if they are from a different group.

4.2 Hypotheses

H1: In the range of reasonable beliefs, second-order beliefs influence actions positively.

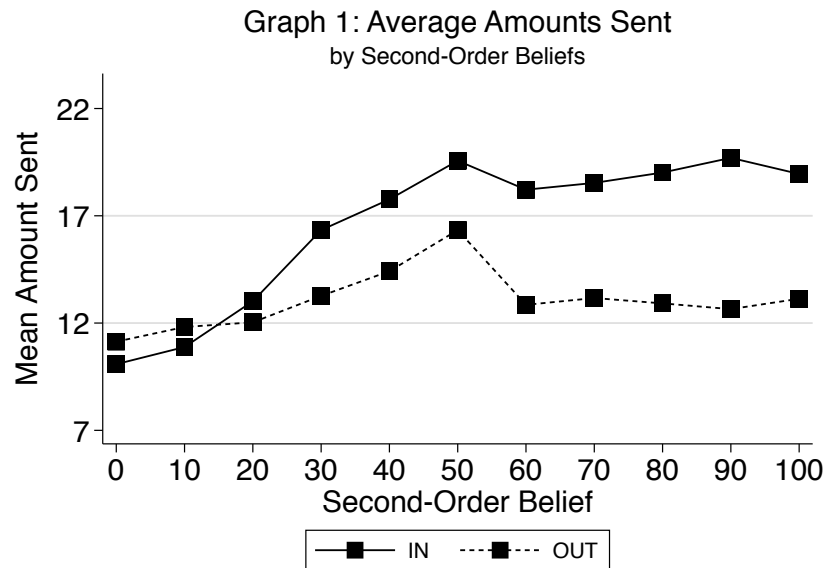
H2: This influence is stronger if the sender and the receiver are from the same group.

In the dictator game played, the range of reasonable beliefs goes from the sender sending 0% of the pie to the sender sending 50% of the pie on average. Shares sent that exceed this range are extremely rare (Engel, 2011) and, accordingly, it is extremely unlikely for receivers to expect the sender to send more than half of the pie.

5. Results

Of the 120 senders, 61 were female and 59 male. 114 senders were students, while 6 were not. The mean age was 24.15, the median age was 23, and the standard deviation 5.18 years.

After briefly exposing my empirical strategy, I test H1 and H2. For the exploration of the interaction effect of second-order beliefs and group identity on action in the realm of exaggerated beliefs, the reader is referred to Appendix A. At the end of this section, I analyze some additional findings on ingroup favoritism, the persistence of group identity, and participants unaffected by second-order beliefs.



5.1 Empirical Strategy

I analyze the decisions that 120 senders took for eleven possible second-order beliefs. 60 senders took their decisions in the outgroup treatment and 60 in the ingroup treatment.

I expect a main effect of the ingroup treatment, which is due to the famous phenomenon of ingroup favoritism (Tajfel, 1979). This effect has to be separated thoroughly from the interaction effect of the ingroup treatment with second-order beliefs, which tests my hypothesis. Therefore I will estimate a linear regression model of the following form with random effects on the participant level. The regression predicts amounts sent using second-order beliefs (sobelief), a dummy for the treatments (ingroup), and the interaction of both (sobelief*ingroup) as independent variables. The ingroup dummy is equal to 1 if the sender and the receiver are from the same group and 0 otherwise. I use a random-effects estimator instead of a fixed-effects estimator, insofar as it does not bias the results away from the result of the fixed-effects estimator. Using a fixed-effects estimator, one of my two main effects (ingroup) would drop out because of a lack of within-subject variance. Therefore the purpose of using random effects is merely to make all effects visible in one model.

$$\text{Amount Sent} = \beta_0 + \beta_1 \cdot \text{sobelief} + \beta_2 \cdot \text{ingroup} + \beta_3 \cdot \text{sobelief} \cdot \text{ingroup} + \text{error (participant random effects)} + \text{error (residuals)}$$

The coefficient β_1 should pick up any effect of second-order beliefs which does not depend on the treatment manipulations. A positive and significant β_1 in the realm of realistic beliefs is evidence in favor of H1. β_2 should pick up the level effect induced by in-group favoritism. And finally, β_3 should pick up whether the ingroup vs. outgroup manipulation reinforces and attenuates the effect of second-order beliefs on action. If β_3 is positive and significant in the realm of realistic beliefs, this is evidence in favor of H2 this paper set out to test.

On top of the simple linear regression model, I will report results from a Tobit regression with random effects at the participant level. It has been shown in the literature that if the experimental design allows not only sending to receivers, but also taking from them, some senders do actually “steal” from receivers’ endowments (Engel, 2011). In my experiment, senders cannot transfer negative amounts. Therefore the data may be censored at zero. Tobit is a common approach to take this censoring into account.

For all random-effects linear regression models, the Hausman test is insignificant. In fact, all coefficients in the linear random effects model are virtually identical to those of the linear fixed effects model.

5.2 Testing the Predictions of Guilt Aversion over Reasonable Beliefs

Graph 1 shows that, in the realm of realistic second-order beliefs (0-50%), the latter influence the amount sent positively in both the ingroup (IN) and the outgroup (OUT) treatment. It is also clearly visible that this influence is stronger ingroup than it is outgroup. Both results are confirmed by the regression analysis summed up in table 1.

Table 1
Data for second-order beliefs from 0 to 50%

Dependent variable: Amount sent	Model 1: Linear regression	Model 2: Tobit regression
Second-order belief	0.100*** (0.0266)	0.141** (0.0431)
Ingroup (dummy)	-1.155 (3.039)	-5.756 (6.172)
Ingroup x Second-order belief	0.104** (0.0377)	0.207*** (0.0623)
Random effects at participant level	Yes	Yes
Constant	10.66***	-2.321
N	720	720
Number of Groups	120	120

*=p<0.05; **=p<0.01; ***=p<0.001
Standard errors in parenthesis

The number of total observations is 720. “Number of groups” refers to the observations grouped by individual participants. The regressions consider amounts sent for six different second-order beliefs per sender (0, 10, 20, 30, 40, 50).

The null hypothesis that in the realm of realistic beliefs second-order beliefs do not influence actions positively has to be rejected. But I am reluctant to interpret this result as general evidence in favor of guilt aversion. By using the strategy method, I basically asked senders to condition their amount sent on second-order beliefs, so it is hard to be surprised that they did.

More importantly, the null that the influence of second-order beliefs is not stronger if the sender and the receiver are from the same group has to be rejected. The “demand” to condition amounts sent on second-order beliefs was constant over treatments, so the positive and significant interaction effect of second-order beliefs and group identity remains valid evidence for the influ-

ence of shared identity on guilt aversion. Accordingly, I derive results one and two.

Result 1: In the realm of realistic beliefs the effect of second-order beliefs on action is positive. (Random effects regression, $p < 0.001$, $\beta_1 = 0.1$).

Result 2: This effect is stronger if senders and receivers share a common group identity. (Random effects regression, $p < 0.01$, $\beta_3 = 0.103$).

My results can also be shown by a Tobit random-effects regression which accounts for the possibility that senders would actually have taken money from receivers if I had let them (Model 2, *tobit random effects*: $\beta_1 = 0.14$, $p < 0.01$; $\beta_3 = 0.2$, $p < 0.001$).

Ex post, the fact that 97.5% of receivers stated beliefs below or equal to 50% of the pie can be regarded as a justification for generating hypotheses only for the realm of realistic beliefs (mode of elicited receiver beliefs: 0% in both treatments; mean overall: 22.30; mean ingroup: 25.35; mean outgroup: 19.26. The difference is marginally significant: Wilcoxon ranksum, $N = 120$, $p = 0.073$).

5.3 Additional Results on Ingroup Favoritism, Unaffected Participants, and the Persistence of Group Identity

It seems striking that the expected level effect of shared group identity is neither visible in the graph nor in the regression analysis. Even searching for differences between the levels of amounts sent by treatment and by second-order beliefs does not provide any statistically significant results (ranksum, all $p > .22$). For very low second-order beliefs (0% and 10%), senders even tend to send less on average to ingroup receivers than to outgroup receivers, although this difference is not statistically significant. Accordingly, the treatment coefficient in the regression is negative.

Result 3 (Null result): The results are inconsistent with general, i.e., belief-independent, ingroup favoritism. For no single level of second-order beliefs do ingroup senders send significantly more than outgroup sender (ranksum, all $p > .22$).

The results also show that in both treatments slightly less than half of the senders are completely unaffected by second-order beliefs. I call a sender “unaffected” if she/he intends to send the same amount for all eleven second-order beliefs offered in the strategy method. Of these participants, there are 29 of 60 in the ingroup treatment and 26 of 60 in the outgroup treatment. One may have expected the amount of unaffected participants to be smaller in the ingroup treatment than in the outgroup treatment because the theory says that second-order beliefs have more effect on action ingroup. But as the utility

function set out above also contains a parameter for individual sensitivity to guilt, the slightly higher amount of unaffected participants in the ingroup treatment could easily be explained by a slightly greater number of insensitive participants who were randomly allocated to the ingroup treatment. In any event, the difference between treatments in unaffected participants is very small and statistically insignificant (*Chi-squared test*, $Chi2=0.3021$, $p>.58$).

I also find that in the ingroup treatment unaffected participants identify less with their group than the affected senders (*Wilcoxon ranksum*, $p<.04$). I measured the degree of group identification as the average score in the four questions of group identification presented to senders in the questionnaire stage (appendix B.3.2b.) In the outgroup treatment, unaffected participants do not distinguish themselves from the affected participants by the degree of group identification (*Wilcoxon ranksum*, $p>.55$). The finding that affectedness and the degree of group identification correlate (only) in the ingroup treatment is in line with the theory set out above: In the outgroup treatment, the identification with one's fellow group members is irrelevant as the senders do not interact with their fellow group members. So only in the ingroup treatment should the degree of identification matter for what senders do. The stronger the identification with one's group in the ingroup treatment, the more second-order beliefs should determine action and the less likely it is that a sender does not react to second-order beliefs at all.

Result 4: In the ingroup treatment, unaffected senders (i.e., senders sending the same amount irrespective of the second-order belief) identify less with their group than affected senders (Wilcoxon ranksum, $N=60$, $p<0.04$). This is not the case in the outgroup treatment (Wilcoxon ranksum, $N=60$, $p>.55$).

A fifth and rather unexpected finding is that the senders' feeling of shared group identity seems to have vanished by the time they have completed their decisions in the game stage. In the posttest, I do not find any treatment difference between the senders' perceived closeness towards their respective receivers. Indeed, the outgroup senders seem to feel slightly closer to their receivers (mean score: 2.56) than ingroup senders seem to do (mean score: 2.53). Given that the treatment manipulation led to normal levels of group identification and does induce a significant difference between ingroup and outgroup treatment, the failure of the closeness measure to pick up a difference between the treatment groups ex post may merely mean that it is not a very reliable measure of group identity. However, it may also indicate that shared minimal group identity decays very quickly, stressing that I chose a very gentle intervention.

6. Discussion and Conclusion

To the best of my knowledge, this experiment is the first to show that, in the realm of realistic beliefs, social closeness – implemented here as shared group identity – determines how strongly senders' second-order beliefs influence the amounts sent in a dictator game. I used a minimal group paradigm to induce a shared identity and reinforced it slightly. The total intervention is extremely faint. Therefore, the effect is likely to be a lot stronger in the field, where relationships are based on family ties, friendship, co-workership, and the like. Also being class mates in an MBA program (Reuben et al., 2009), exchanging a one page letter (Charness & Dufwenberg, 2006) or being parties to a promise arising in a computer chat (Vanberg, 2008) are protocols that are likely to induce stronger shared identity than my treatment manipulation.

I further find that if senders and receivers are from the same group, those senders who previously stated that they identify strongly with the group are more likely to be affected by second-order beliefs. This further corroborates my main result that shared identity determines the effect of second-order beliefs on action.

My results clarify that guilt aversion will make better predictions in contexts of social closeness (families, friendships, co-workers) than in anonymous contexts (anonymous market transactions). On the one hand, they reveal that experiments in a very anonymous setting may be the wrong test bed to test theories of second-order belief-dependent preferences. On the other hand, my results suggest that theories on guilt aversion should spell out that social closeness is crucial for the effect of second-order beliefs on action.

In my experiment, a general level effect of ingroup favoritism is absent. Holding second-order beliefs fixed, I cannot find ingroup favoritism for any single level of second-order beliefs. This is in line with what Güth et al. (2009) and Ockenfels & Werner (in press) have suggested: Ingroup favoritism possibly does depend on second-order beliefs, such that the difference in amounts sent ingroup and outgroup are due to a higher level of second-order beliefs in ingroup interactions. Also in line with Güth et al. (2009) and Ockenfels & Werner (in press), I show that shared group identity does indeed translate into elevated expectations of receivers – which senders may well anticipate. But beyond Güth et al. (2009) and Ockenfels & Werner (in press), I also show in this experiment that their explanation may at least be incomplete. I show that shared group identity not only raises receivers' expectations, but leads to a stronger influence of the senders' second-order belief actions. Both effects together may just reinforce each other. However, according to my results, the increase in expectations required to trigger ingroup favoritism may be smaller than implied by Güth et al. (2009) and Ockenfels et al. (in press). In fact, my results suggest that ingroup favoritism independent of second-order belief re-

mains possible in case of a strong shared identity. In case of very strong shared identity, ingroup favoritism could arise only through the stronger impact of a fixed level of second-order beliefs. This would mean that ingroup favoritism was possible under identical second-order beliefs ingroup and outgroup. Güth et al. (2009) and Ockenfels & Werner (in press) would not make this prediction. My experiment, which only induced group identity very gently, could not test this prediction conclusively. But this test appears to be a promising avenue for future research.

From my results it appears plausible that people hold a promise to a stranger because the promise creates a shared identity between the two, causing second-order beliefs to induce action. In future research it should be tested whether promise keeping can be better predicted by a theory of guilt aversion amended along the lines described here or by a preference to hold a promise. Promises that activate guilt aversion by creating a relationship between the parties would be compatible with a theory of “lexicographic promise keeping” proposed by Ederer and Stremitzer (2014).

The finding that people have a preference to conform to the expectations of someone who is socially close may have applications in the management of teams. Guilt aversion can help coordinate team members. Communicating expectations can incite team members who are socially close. At least in the realm of realistic expectations, the degree of social integration of a team can be used as a mediator to fine-tune the influence of mutual expectations. It seems like an interesting and promising avenue for future research to enrich the investigation of the impact of social closeness on guilt aversion by the impact of social status.

Finally, my results suggest that it is worth working on a truly empathic utility function that does not merely include other agents’ payoffs into the utility function, but adds more elements of their utility. A theory of other-regarding reference-dependent preference with expectation-based reference points, along the lines of Köszegi and Rabin (2006), appears to be a promising starting point.

7. References

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8. Appendix A: Exploratory Results Regarding Unrealistic Beliefs

8.1 Predictions:

For exaggerated beliefs, this study is exploratory. In this range, conflicting forces are likely, making it difficult to derive one clear-cut hypothesis. The first force may be guilt aversion in the traditional sense. Even in the realm of exaggerated beliefs, senders may increase their amounts sent in response to increasing second-order beliefs. If beliefs are exaggerated, however, senders may also negatively condition their amounts sent on beliefs, “punishing” exaggerated beliefs. Senders may actually do so more, the more exaggerated the beliefs are. Regner and Harth (2010) found evidence that the more exaggerated beliefs are, the less trustees send in a trust game. But they explained their findings with reciprocity dominating guilt aversion in that domain. Theories of reciprocity do not – at least in their traditional form (Dufwenberg & Kirchsteiger, 2004; Falk & Fischbacher, 2006; Rabin, 1993) – apply to my design. The receiver does not act. So the sender cannot reciprocate on any kind or unkind action.

However, the “punishment” of exaggerated beliefs could also be flat. This would mean senders do discount their amount sent if beliefs are exaggerated and would send the same low amount for all exaggerated beliefs. Finally the senders would have good reason just to ignore exaggerated beliefs. All these forces may interact with a shared group identity. But again the signs of these interaction effects seem unclear. There is evidence that participants tend to be more forgiving towards people who share their group identity (Chen & Li, 2009, p. 445). But an exaggerated belief could also be interpreted as particularly presumptuous if it is stated from a group-mate meriting harsher “punishment”.

8.2 Results

The regression analysis set out in the main body of the paper does not change qualitatively when including the full range of second-order beliefs, (*Random effects regression*, $N=120$; $\beta_3=0.08$, $p=0.000$, see below table 2, model 1). But it is obvious from plotting the average amounts sent against the whole range of second-order beliefs by treatment (Graph 1) that senders react differently to reasonable second-order beliefs than they react to exaggerated second-order beliefs. To explore the data in the realm of exaggerated beliefs, I amend the original regressions by including a dummy for exaggerated beliefs I call “larger50”. The dummy is equal to 1 for those amounts sent that are conditioned on the receiver’s expectation that the sender sends more than half of the pie. For amounts sent conditioned on the receiver expecting not more than half of the pie, the dummy is 0. β_4 denotes the coefficient of this dummy. Including this dummy confirms the observation that senders reduce the amount sent once second-order beliefs start being exaggerated (*Random effects regression*, $N=120$; $\beta_4=-3.563$, $p=0.001$). Adding an interaction effect of the larger50 dummy with the ingroup dummy into this regression reveals that this reduction is not stronger in statistically significant terms if senders send to outgroup members (*Random effects regression*, $N=120$; $\beta_5=-0.346$, $p=0.875$). Again, the results can also be shown using the Tobit random effects model also introduced in the main body of the paper.

Table 2

All Data: second-order beliefs 0-100%

Dependent variable: Amount sent	Model 1: Linear regression	Model 3: Linear regression	Model 4: Linear regression	Model 2: Tobit re- gression	Model 5: Tobit re- gression	Model 6: Tobit re- gression
Second-order belief	0.0129 (0.0124)	0.0615** (0.0195)	0.063** (0.025)	-0.0142 (0.0221)	0.0837* (0.0347)	0.084+ (0.0437)
Ingroup (dummy)	-0.524 (3.371)	-0.524 (3.370)	-.445 (3.407)	-5.515 (7.204)	-5.572 (7.200)	-5.525 (7.26)
Ingroup x Second- order belief	0.0802*** (0.0175)	0.0802*** (0.0174)	0.075* (0.035)	0.157*** (0.0316)	0.158*** (0.0314)	0.155* (0.0773)
Larger50 (dummy)		-3.563** (1.105)	-3.39* (1.563)		-7.185*** (1.982)	-7.086* (2.816)
Larger50 x ingroup 1 0			-0.346 (2.211)			-0.195 (3.962)
Random effects at participant level	Yes	Yes	Yes	Yes	Yes	Yes
Constant	12.42*** (2.384)	11.61*** (2.396)	11.57*** (2.409)	-2.938 (5.166)	-4.578 (5.183)	-4.59 (5.202)
N	1320	1320	1320	1320	1320	1320
Number of groups	120	120	120	120	120	120

+=p<0.06 *=p<0.05; **=p<0.01; ***=p<0.001

Standard errors in parenthesis

Result 5: The effect of second-order beliefs is attenuated once the realm of exaggerated beliefs is reached (Random effects regression, N=120; $\beta_4 = -3.39$, $p < .05$).

(Null) Result 6: This attenuation is not different between treatments (Random effects regression, N=120; $\beta_5 = -0.346$, $p = .87$).

To look closer at whether and, if so, how senders condition amounts sent on exaggerated beliefs, I run the regressions explained in the main body of the paper with the data on exaggerated second-order beliefs.

Table 3		
Data for second-order beliefs above 50 up to 100%		
Dependent variable: Amount sent	Model 1: Linear re- gression	Model 2: Tobit re- gression
Second-order belief	0.0005 (0.02)	0.0005 (0.02)
Ingroup (dummy)	3.873 (0.79)	3.873 (0.8)
Ingroup x Second- order belief	0.026 (0.76)	0.026 (0.76)
Random effects at participant level	Yes	Yes
Constant	12.9*** (3.73)	12.9*** (3.75)
N	600	600
Number of groups	120	120
*=p<0.05; **=p<0.01; ***=p<0.001 Standard errors in parenthesis		

With these regressions, no significant effect can be shown. It appears that, in the realm of excessive second-order beliefs, second-order beliefs do not have any effect on the amount sent – independently of whether senders interact with ingroup or outgroup receivers.

(Null) result 6: In the realm of exaggerated beliefs, no influence of second-order beliefs on action can be shown. That is true independently of whether senders interact with ingroup or outgroup receivers (Random effects regression, N=120; $\beta_1=0.0005$, $p>.983$; $\beta_3=0.026$, $p>.45$).

8.3 Conclusion

In the exploratory part of the experiment, I find that exaggerated second-order beliefs generally have an attenuated influence on the amount sent. The difference of attenuation is not statistically significant between treatments. Within the realm of exaggerated beliefs, second-order beliefs seem to have no effect on the amount sent, independent of the treatment.

9. Appendix B: Instructions in English

B.1. General Instructions (on Paper)

Welcome to our experiment!

If you read the following explanations carefully, you will be able to earn a substantial sum of money, depending on the decisions you take. It is therefore crucial that you read these explanations carefully.

During the experiment there shall be absolutely no communication between participants.

Any violation of this rule will lead to your exclusion from the experiment and from any payments. If you have any questions, please raise your hand. We will then come over to you. Please switch you mobile phone off and do not listen to music during the experiment.

In any event, you will receive a lump sum of 4 € for taking part in the experiment. During the experiment, all payoffs and earnings will be expressed in Talers. At the end of the experiment, you will be paid cash in Euro. You will receive from us the 4 € for your participation plus the sum of Talers you earned in the experiment, converted into Euros. One Taler converts to 0.11 €. Today's experiment will consist of three parts. Before each part, the experimenter will hand you printed instructions. Now you are about to be instructed on the first phase. When the first phase is over, you will receive paper instructions on the second phase of the experiment.

B.2. Part 1 = Group Segregation Stage (Instructions on Paper)

- All participants will now be allocated to one of two groups. The groups will remain constant over the whole experiment (that is, over all three parts of today's experiment).
- The groups will be formed depending on your preferences for one of two modern painters.
- To form the two groups, everyone will be shown 5 pairs of paintings. Each pair consists of one painting by Klee and one by Kandinsky. You will not be told who painted which painting. For each pair, you will be asked to rate how much you like one painting vis-à-vis the other.
- For indicating your preference, we ask you to use a slider bar. You can move the slider bar on a continuous scale between "I strongly prefer

painting L” to “I strongly prefer painting R” to indicate your preference. The middle position shows that you like both paintings equally.

- By moving the slider bar, you distribute ten points between the two paintings. The more you indicate that you like a painting, the more points are allocated to this painting and the less to the other (“I strongly prefer painting L” means 10 points for painting L; “I like both paintings equally” means 5 points for both paintings; “I strongly prefer painting R” means 10 points for painting R).
- The computer sums up all points you allocated to Klee and all points you allocated to Kandinsky. Then the half of the participants who allocated the most points to Klee will form the Klee group. Accordingly, the other half will form the Kandinsky group.
- As of the end of this stage, you will be informed about your group membership.
- You can read your group membership from the upper left corner of your screen at any time during today’s experiment.
- The groups will remain constant over all three stages of today’s experiment.

B.3. Part 2

B.3.1. Group Task (Instructions on Paper)

- You will approach this task together with the members of your group (Kandinsky or Klee). Each member will work independently, but the performance of all group members will be aggregated and constitutes a joint group performance.
- The performance of your group will be compared with the performance of the other group.
- The group with the higher performance will receive a prize of 208 Talers at the end of the experiment, which will be distributed equally among the eight group members (26 Talers per member). The group with the lower performance receives no prize. If both groups have exactly the same performance, the prize will be shared equally between the groups.
- Your task is to identify letters at certain positions in a 15-page text.
- Example: Identify the following letter: page 1, line 7, word 5, position 3. The correct solution to this example is marked in grey in the text you received – it is letter „C“.
- Please indicate all letters as capitals.

- Overall you have four minutes to identify as many letters as possible.
- After four minutes, the task ends and the number of correct solutions in your group will be compared with the number of correct solutions in the other group.
- At the end of the experiment (after the third part), you will be informed which group wins the price.
- If you have read and understood the instructions, please click on “Proceed” on your computer screen.
- As soon as all participants have clicked on “Proceed”, the group task will start. Please be ready!
- Once you have completed this task, we will ask you to answer a short questionnaire, which will appear on your screen automatically.

B.3.2a. Belief Elicitation (Only on Screen, Only for Receivers)

Before we proceed with the third part of the experiment, we want you to guess the outcome of it. You will act in role B. In the experiment, you will be anonymously paired with another person who has role A. The only thing you and the person you are paired with will know about each other is to what groups you have been assigned– Kandinsky or Klee.

The person you are paired with will decide how to split 100 Talers between himself/herself and you. Every individual decision by such a person in role A will be anonymous towards both other participants and the experimenters. We want you to guess how much, on average, of the 100 Talers a person in your situation (a person in role B matched with a person of the same / different group in role A) will receive. Please enter your guess in the box below, stated in full Talers. Each participant whose guess is not more than 1 Taler off the true average amount will win 125 Talers extra, which will be paid out in the end together with whatever you earned during this experiment.

B.3.2b. Questionnaire (Only on Screen, Only for Senders).

You will now read some statements. These statements refer to the Klee [Kandinsky] group, of which you are a member. Please read the respective statement carefully and then indicate to which extent you agree with it. You can click anything between 1 (“I do not agree at all”) and 7 (“I absolutely agree”).

Example scale: I do not agree at all. 1 - 2 - 3 - 4 - 5 - 6 - 7 I absolutely agree.

- 1) I regard myself as a member of the Klee [Kandinsky] group.
- 2) I am happy about being a member of the Klee [Kandinsky] group.
- 3) I feel somehow connected to the members of the Klee [Kandinsky] group.
- 4) I identify myself as a member of the Klee [Kandinsky] group.

B.4. Part 3 = Dictator Game

- Each of you has been paired with another person in another role. You can read your role (“A” or “B”) on your screen. You will not be told who this other person is, neither during nor after the experiment.
- All you will know about this person is what group he/she belongs to (Klee/Kandinsky). You can read your own group membership on the top left of your screen at any time. You can read the group membership of the person you have been paired with from the top right of your screen at any time.
- In this part of the experiment, every person who has role A will decide how to divide 100 Talers between himself/herself and the person in role B with whom he/she has been paired. This will work as follows.
- 100 Talers each will be booked to the experimental accounts of every participant in role A.
- Every participant in role B has guessed the outcome of this experiment to be in a case like his/hers:
 - If you and the person you have been paired with are from the same group, he/she guessed the average amount a participant in role B will receive if the participant in role A he/she is paired with is for same group.
 - If you and the person you have been paired with are from different groups, he/she guessed the average amount a participant in role B will receive if the participant in role A he/she is paired with is for a different group.
- Note: This guess was made before these instructions were handed out (during the questionnaire at the end of the last part) and without the participant in role B knowing that the participant in role A he/she was paired with would be informed about the guess. Every person in role B whose guess is not more than 1 Talers off the true average will receive 125 Talers to provide an incentive to guess accurately.
- The participant in role A will now be asked what they would like to send if the receiver has guessed 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, or 100 Talers, respectively. Please fill in an answer for all these eleven cases.
- The computer will then put into effect the answer that was conditional on the belief, which is closest to the person B’s actual stated guess. So, if the person in role B guessed a participant in his situation would receive 4 Talers on average, the answer for 0 Talers would be put into effect. And if the person in role B guessed a participant in his situation

would receive 96 Talers on average, the answer for 100 Talers would be put into effect.

- After the experiment you will be informed about
 - How much you earned in the second stage of the experiment (group competition and belief elicitation)
 - How much you earned in the third stage of the experiment (sender receiver game).
- After completion of this last part of the experiment, we would ask you please to fill in a general questionnaire while we calculate your payments. Please step forward one by one in the order of your cabin numbers as soon as the experimenter declares the experiment to be over.

B.5. Posttest

In a posttest, participants were asked to indicate which of the pairs of circles best describes their relationship to the participant they have been paired with in the dictator game.

<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>
<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>
<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>
<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>
<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>
<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>
<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">Sie</div> <div style="border: 1px solid black; border-radius: 50%; width: 40px; height: 40px; display: flex; align-items: center; justify-content: center;">der Andere</div> </div>

10. *Appendix C: Instructions in German*

Instruktionen auf Papier

Allgemeine Erklärungen für die Teilnehmer

Willkommen zu unserem Experiment!

Wenn Sie die nachfolgenden Erklärungen genau lesen, dann können Sie - je nach Ihren Entscheidungen - eine nicht unbeträchtliche Geldsumme verdienen. Es ist daher sehr wichtig, dass Sie diese Erklärungen genau durchlesen.

Während des Experiments herrscht ein absolutes Kommunikationsverbot mit den anderen Teilnehmern. Die Nichtbeachtung dieser Regel führt zum Ausschluss vom Experiment und allen Zahlungen. Wenn Sie Fragen haben, strecken Sie bitte Ihre Hand aus der Kabine. Wir kommen dann zu Ihnen. Bitte schalten Sie Ihr Handy aus und hören Sie keine Musik.

Für Ihre Teilnahme am Experiment erhalten Sie auf jeden Fall eine Pauschale von 4 Euro.

Während des Experiments sprechen wir nicht von Euro, sondern von „Talern“. Ihr gesamtes Einkommen wird also zunächst in Talern berechnet. Die von Ihnen während des Experiments erzielte Gesamtpunktzahl wird dann am Ende in Euro umgerechnet. Dabei gilt:

1 Taler = 0.11 Euro

Am Ende bekommen Sie von uns die 4 Euro Pauschale sowie die während des Experiments verdiente Anzahl an Talern **bar** in Euro ausbezahlt.

Das heutige Experiment besteht aus drei Teilen. Vor jedem Teil werden Sie Instruktionen auf Papier erhalten. Nun wird Ihnen der erste Teil erklärt. Wenn der erste Teil vorüber ist, erhalten Sie die Instruktionen über den zweiten Teil auf Papier. Nach dem zweiten Teil erhalten Sie die Instruktionen für den dritten Teil auf Papier

Ablauf des ersten Teils

- Im ersten Teil des Experiments werden alle Teilnehmer einer von zwei Gruppen zugeteilt. Die Gruppen werden über das ganze Experiment, d.h. über alle drei Teile des Experiments, konstant bleiben.
- Die Gruppen werden nach Ihrer Vorliebe für einen von zwei modernen Malern gebildet.
- Um die Gruppen zu bilden, werden wir jedem von Ihnen 5 Paare von Gemälden zeigen. Jedes Paar wird aus je einem Bild von Klee und einem von Kandinsky bestehen. Sie werden nicht erfahren, welches Bild von welchem Maler stammt. Für jedes Bilderpaar werden Sie gefragt, wie sehr sie das eine Bild im Vergleich zum anderen mögen.
- Um Ihre Vorliebe anzugeben, werden wir Sie bitten, einen Schiebe-Regler auf dem Bildschirm zu nutzen. Sie können den Schiebe-Regler auf einer kontinuierlichen Skala zwischen „Mir gefällt Bild L viel besser“ bis „Mir gefällt Bild R viel besser“ bewegen. Die Position des Schiebe-Reglers genau in der Mitte zwischen diesen beiden Enden bedeutet, dass Sie die beiden Bilder gleich stark schätzen.
- Indem Sie den Schiebe-Regler auf der Skala bewegen, verteilen Sie zehn Punkte auf die beiden Bilder. Je mehr sie ein Bild nach Ihrer Angabe mögen, desto mehr Punkte werden diesem Bild und desto weniger Punkte werden dem anderen Bild zugeteilt
Beispiel: „Mir gefällt Bild L viel besser“ bedeutet 10 Punkte für Bild L und 0 Punkte für Bild R; „Mir gefallen beide Bilder gleich“ bedeutet 5 Punkte für beide Bilder und „Mir gefällt Bild R viel besser“ bedeutet 0 Punkte für Bild L und 10 Punkte für Bild R.
- Der Computer wird alle Punkte, die Sie den Bildern von Klee gegeben haben, aufsummieren. Ebenso wird er alle Punkte, die Sie den Bildern von Kandinsky gegeben haben, aufsummieren. Dann wird die Hälfte der Teilnehmer, die die meisten Punkte an Klee-Bilder gegeben haben, die Klee-Gruppe bilden. Entsprechend wird die andere Hälfte der Teilnehmer die Kandinsky-Gruppe bilden.
- Am Ende dieses Teils des Experiments wird Ihnen mitgeteilt, zu welcher Gruppe Sie gehören.

- In der oberen linken Ecke des Bildschirms werden Sie von nun an während des ganzen Experiments darüber informiert, zu welcher Gruppe Sie gehören.
- Die Gruppen bleiben über alle drei Teile des heutigen Experiments unverändert.

Ablauf des zweiten Teils

- Die Aufgabe in diesem Teil werden Sie zusammen mit den anderen Mitgliedern Ihrer Gruppe (Klee bzw. Kandinsky) angehen. Jedes Gruppenmitglied wird unabhängig handeln, aber die Leistungen aller Mitglieder einer Gruppe werden zu einer gemeinsamen Gruppenleistung zusammengekommen.
- Die Leistung Ihrer Gruppe wird dann mit der Leistung der anderen Gruppe verglichen.
- Die Gruppe mit der besseren Leistung wird einen Preis von 208 Talern gewinnen, der gleichmäßig auf alle acht Mitglieder der Gruppe aufgeteilt wird. Die Gruppe mit der schlechteren Leistung erhält nichts. Wenn die Leistung beider Gruppen gleich ist, wird der Preis zwischen beiden Gruppen geteilt.
- Sie werden einen 15-seitigen Text erhalten und Ihre Aufgabe wird darin bestehen, Buchstaben an einer konkreten Position zu bestimmen.
Beispiel: „Bestimmen Sie den folgenden Buchstaben: Seite 1, Zeile 7, Wort 5, Position 3.“ Die richtige Antwort zu diesem Beispiel ist in dem Text, den Sie erhalten haben, grau unterlegt – es ist der Buchstabe „C“.
- Bitte geben Sie alle Buchstaben in Großbuchstaben an.
- Insgesamt haben Sie 4 Minuten, um so viele Buchstaben zu identifizieren wie möglich.
- Nach 4 Minuten endet die Aufgabe und die Anzahl richtiger Antworten in Ihrer Gruppe wird mit der Anzahl richtiger Antworten der anderen Gruppe verglichen.
- Am Ende des Experiments (nach dem dritten Teil) werden Sie informiert, welche Gruppe die meisten richtigen Antworten gegeben und damit den Preis gewonnen hat.
- Wenn Sie diese Instruktionen gelesen und verstanden haben, klicken Sie „Weiter“ auf Ihrem Bildschirm.
- Sobald alle Teilnehmer „Weiter“ geklickt haben, wird die Aufgabe beginnen. Halten Sie sich bereit!
- Wenn diese Aufgabe beendet ist, werden wir Sie bitten, einen kurzen Fragebogen auszufüllen, der automatisch auf Ihrem Bildschirm erscheinen wird.

Ablauf des dritten Teils

- Sie wurden am Ende des letzten Teils des Experiments zufällig entweder der Rolle „A“ oder der Rolle „B“ zugeordnet.
- Welche der beiden Rollen Ihnen zugeteilt wurde, wird Ihnen auf dem Bildschirm mitgeteilt, sobald der dritte Teil des Experiments beginnt.
- Jeder von Ihnen wurde mit jeweils einer Person in einer anderen Rolle gepaart.
- Sie werden weder während noch nach dem Experiment erfahren, wer die Person ist, mit der Sie gepaart wurden.
- Alles, was Sie über diese Person wissen werden, ist, welcher Gruppe (Klee oder Kandinsky) sie angehört.
- Sie können Ihre eigene Gruppenzugehörigkeit jederzeit von der linken oberen Ecke Ihres Bildschirms ablesen. Sie können die Gruppenzugehörigkeit der Person, mit der Sie gepaart wurden, jederzeit von der rechten oberen Ecke Ihres Bildschirms ablesen.
- In diesem Teil des Experiments wird jede Person in der Rolle „A“ entscheiden, wie sie 100 Taler zwischen sich selbst und der Person, mit der sie gepaart ist, aufteilen wird. Dies wird wie folgt funktionieren.
- 100 Taler werden jeweils auf das Konto jeder Person in Rolle „A“ gebucht.
- Jeder Teilnehmer in der Rolle „B“ hat geschätzt, wie diese Aufteilung in einem Fall wie dem Ihren ausgehen wird:
 - Falls Sie und die mit Ihnen gepaarte Person aus der selben Gruppe sind, hat die Person in Rolle „B“, folgende Frage beantwortet: Wie viel wird eine Person in Rolle „B“ im Durchschnitt erhalten, wenn sie mit einer Person in Rolle „A“ gepaart wurde, die aus der selben Gruppe (Klee/Kandinsky) stammt wie sie selbst.
 - Falls Sie und die mit Ihnen gepaarte Person aus unterschiedlichen Gruppen stammen, hat die Person in Rolle „B“, folgende Frage beantwortet: Wie viel wird eine Person in Rolle „B“ im Durchschnitt erhalten, wenn sie mit einer Person in Rolle „A“ gepaart wurde, die aus einer anderen Gruppe (Klee/Kandinsky) stammt als sie selbst.
- Beachten Sie: Diese Schätzung hat die Person in Rolle „B“ gemacht, bevor diese Instruktionen ausgeteilt wurden (während der Fragebogenphase im letzten Teil des Experiments). Die Teilnehmer in Rolle „B“ wussten nicht, dass der Teilnehmer in Rolle „A“ über ihre Schätzung informiert wird. Je-

de Person in Rolle „B“, deren Schätzung nicht mehr als einen Taler vom wirklichen Durchschnitt entfernt liegt, wird 125 Taler erhalten. Das sollte einen Anreiz bieten, eine zutreffende Schätzung abzugeben.

- Die Teilnehmer in Rolle „A“ werden nun gefragt, welchen Betrag sie dem jeweiligen Teilnehmer in Rolle „B“ senden wollen, falls der Teilnehmer in Rolle „B“ 0, 10, 20, 30, 40, 50, 60, 70, 80, 90 bzw. 100 Taler geschätzt hat. Bitte antworten Sie für **jeden** dieser Fälle.
- Der Computer wird dann die Antwort für den Fall umsetzen, der am nächsten an der wirklichen Schätzung der Person in Rolle „B“ liegt.
Beispiel: Falls die Person in Rolle „B“ geschätzt hätte, dass eine Person in ihrer Situation im Durchschnitt vier Taler empfangen würde, würde der Computer die Antwort der Person in Rolle „A“ für den Fall „0 Taler“ umsetzen. Falls die Person in Rolle „B“ geschätzt hätte, dass eine Person in ihrer Situation im Durchschnitt 96 Taler erhalten würde, würde der Computer die Antwort der Person in Rolle „A“ für den Fall „100 Taler“ umsetzen
- Hiernach stellen wir Ihnen noch eine Frage.
- Danach werden Sie darüber informiert,
 - wie viel Sie im zweiten Teil des Experiments verdient haben (Gruppenaufgabe und Schätzung)
 - wie viel Sie im dritten Teil des Experiments verdient haben (Sender-Empfänger-Aufgabe)
 - wie viel Sie insgesamt verdient haben.

Bitte beantworten Sie nach dem letzten Teil des Experiments noch einen allgemeinen Fragebogen, während wir Ihre Auszahlung berechnen. Bitte kommen Sie dann einzeln in der Reihenfolge Ihrer Kabinenummern zum Auszahlungstisch, wenn der Leiter des Experiments Sie dazu auffordert.

Beliefabfrage (nur auf dem Bildschirm, nur für Empfänger)

Bevor wir zur dritten Phase des Experiments kommen, möchten wir Sie bitten, das Ergebnis der dritten Phase vorherzusagen.

Dazu erklären wir Ihnen schon hier, was in der dritten Phase des Experiments geschehen wird.

Ihnen ist für die dritte Phase die Rolle des B-Spielers zugewiesen worden. Ferner wurden Sie für die dritte Phase schon zufällig einem anderen Teilnehmer in der Rolle A zugeordnet. Die einzige Information, die Sie über den anderen Teilnehmer haben werden, ist, ob dieser zur Klee- oder zur Kandinsky-Gruppe gehört. Ebenso wird er über Sie nur wissen, ob Sie zur Klee- oder Kandinsky-Gruppe gehören.

Sie gehören zur Kandinsky-Gruppe [Klee-Gruppe].

Der Ihnen zugeteilte Teilnehmer in Rolle A gehört zur Kandinsky-Gruppe [Klee-Gruppe].

Der Teilnehmer in Rolle A, dem Sie zugeordnet sind, wird darüber entscheiden, wie 100 Taler zwischen Ihnen beiden aufgeteilt werden.

Wir bitten Sie, zu schätzen, wie viel von den 100 Talern ein Teilnehmer in Rolle B im Durchschnitt von einem Teilnehmer in Rolle A erhalten wird, wenn er sich in einer Paarung wie der Ihren befindet (beide Teilnehmer gehören der selben Gruppe an [die beiden Teilnehmer gehören unterschiedlichen Gruppen an]).

Jeder Teilnehmer, dessen Schätzung ausreichend präzise ist, erhält 125 Taler, die am Ende des Experiments zusammen mit dem Betrag ausgezahlt werden, den er während des Experiments verdient hat. Ausreichend präzise ist Ihre Schätzung, wenn sie nicht mehr als 1 Taler vom „wahren Durchschnittswert“ entfernt liegt. Der „wahre Durchschnittswert“ ist der durchschnittliche Betrag, den Teilnehmer in Rolle B in einer Paarung wie der Ihren (beide Teilnehmer gehören derselben Gruppe an [die beiden Teilnehmer gehören unterschiedlichen Gruppen an]) in dieser Session von Teilnehmern in Rolle A erhalten.

Bitte geben Sie Ihre Schätzung in das Feld unter diesem Text ein. Sie können jeden Betrag zwischen 0 und 100 Talern angeben, der sich in ganzen Talern ausdrücken lässt.

Ein Teilnehmer in Rolle B der - wie in Ihrem Fall – zur selben Gruppe gehört wie [zu einer anderen Gruppe gehört als] der Teilnehmer in Rolle A, der ihm zugeordnet ist, erhält im Durchschnitt ... Taler.

Fragebogen auf dem Bildschirm für die Sender).

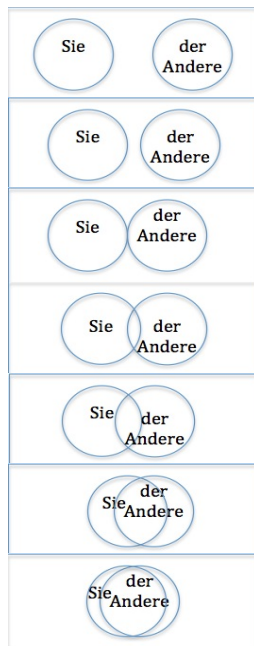
Sie werden nun einige Aussagen lesen. Die Aussagen beziehen sich auf die Klee [Kandinsky] -Gruppe, der Sie angehören. Lesen Sie sich die jeweilige Aussage genau durch und klicken Sie an, wie sehr Sie ihr zustimmen oder nicht zustimmen. Dabei können Sie zwischen 1 "stimme überhaupt nicht zu" und 7 "stimme absolut zu" wählen.

Beispielskala: stimme überhaupt nicht zu 1 - 2 - 3 - 4 - 5 - 6 - 7 stimme voll zu

- 1) Ich sehe mich selbst als Mitglied der Klee [Kandinsky]-Gruppe
- 2) Ich bin froh darüber, zur Klee [Kandinsky]-Gruppe zu gehören.
- 3) Ich fühle mich irgendwie den anderen Mitgliedern der Klee [Kandinsky]-Gruppe verbunden.
- 4) Ich identifiziere mich als Mitglied der Klee [Kandinsky]-Gruppe.

Posttest

Im Posttest wurden die Teilnehmer gebeten, das Kreispaar anzuklicken, das am ehesten ihre Beziehung zu dem mit ihnen für das Diktatorspiel gepaarten Teilnehmer beschrieb.



IV. Partnerships and Consortia: The Effect of Sharing Rules on Oligopolistic Pricing

1. *Introduction*¹³

Agents in economic models are typically modeled as monolithic decision makers. While this is a useful simplifying assumption, it is in contrast with the observation that many real world economic decisions are made by groups or teams. Boards of directors determine the behavior of firms, and self-monitored profit-sharing teams characterize many industries for artistic and professional services, e.g., lawyers, consultants, or music bands.

Modeling economic agents as teams is important because team dynamics and decisions, and subsequently the outcomes of inter-team interactions and markets in which teams operate, can be crucially influenced by the teams' internal organization. In this paper we experimentally study how team's internal organization, operationalized as the way profits are divided among team members, affects the unfolding of duopoly Bertrand competitions.

Suppose that a state agency wishes to sue a construction company, claiming back aid granted for the construction of a power plant. The state decides to auction off the mandate in a public procurement auction, where the applicant who submits the lowest asking price wins the auction. Applicants need to bring in expertise in dispute resolution because it is a court case; the embeddedness of the case in the energy sector requires expertise in energy law; and finally the core of the case certainly lies in the law of state aid. Since such a wide array of qualifications is beyond the scope of any single lawyer, a team of lawyers submits a joint tender, competing against other teams, and the team with the lowest asking price wins the project and is paid its asking price.

A straightforward way to model the pricing decision in the above scenario is to assume that each expert in the team states a personal asking price and the joint bid is the sum of these asking prices. Clearly, team members have a joint interest in winning the project by asking for prices that are lower, in sum, than those of the competitors. However, depending on the way profits are divided

¹³ We thank Oliver Kirchkamp, Botond Köszegi, Christoph March, Ivan Soraperra and Bert Willems for helpful comments on earlier versions of this paper.

among members of the winning team, they can also have conflicting interests; given the public good flavor of low joint bids, if team members receive their own personal bids when the team wins, then each one would prefer her teammates to bid low while bidding high herself to maximize her profits.

We consider two ways of dividing profits among members of the winning team in a Bertrand price competition: (1) each member of the winning team receives her personal asking price; and (2) each member of the winning team receives the average personal asking prices in the team (an equal share of the team's profit). It is clear that the internal conflict within the team is pronounced in (1) and absent in (2). Relating these two incentive structures to the example above and to real teams in the legal domain, two (stylized) types of competitors can apply to take the mandate: *consortia* and *partnerships*. Both consortia and partnerships are composed of a number of lawyers, each with one of the required specializations. In a consortium it is common that each lawyer fixes her own hourly rate, and is paid – in case the consortium gets the mandate – according to this rate. On the other hand, in partnerships the usual practice is that all partners agree on identical hourly rates for all partners, and some even place yearly profits in a pot to be distributed equally or at least at fixed ratios among all partners at the end of the year.

We experimentally examine both homogeneous duopolies composed of either two consortia or two partnerships, and heterogeneous duopolies composed of one consortium and one partnership. Additionally, we vary the transparency of the profit sharing arrangements, i.e., whether team members have information about the profit sharing method of the other team or not. In summary, our results show that (1) Homogenous consortia markets yield substantially higher prices than homogeneous partnership markets, both when profit sharing arrangements are transparent and when they are intransparent. (2) When profit sharing arrangements are transparent, prices in heterogeneous markets are as low as prices in homogenous partnership markets. (3) When profit sharing arrangements are intransparent, prices in heterogeneous markets are (almost) as high as prices in homogenous consortia markets. (4) Transparency of profit sharing arrangements leads to higher prices in homogeneous markets but to lower prices in heterogeneous markets.

Our results can assist teams in forming preferences about their own profit sharing rule and transparency policy and about the types of markets they choose to compete in, as well as inform market regulators in the design of trading institutions (as all determinants of price levels are relevant for the efficient distribution of goods) or in forecasting (tacit) collusion, which is usually

thought of as being attained by coordination on prices, but may also be attained by coordination on less competitive internal structures.

In the next section we relate our work to existing literature. Then we present our experimental design and procedures. Subsequently we derive testable hypotheses. Section four presents the results of the experiment. Section five concludes the paper and provides recommendations for market participants and policy makers.

2. Related literature and current contribution

The current work mainly relates to two streams of literature: work on teams and their optimal organization; and (experimental) work on contests in economics settings. In this section we briefly mention a number of key results from each stream, and explain how our work relates to, and expands upon, both.

Alchian & Demsetz (1972) point out that in certain professions—law firms, for example—organizing production by establishing profit-sharing teams, rather than via central management, can increase production efficiency by circumventing the need to centrally monitor individual efforts.¹⁴ In a similar vein, profit-sharing among team members provides insurance against idiosyncratic shocks to human capital (Lang & Gordon, 1995) and helps committing to high quality when it cannot be easily assessed by customers (Levin & Tadelis, 2005). A related body of work deals with the way profits are shared. In particular, the commonly employed equal profit sharing rule has been shown to have both advantages and disadvantages: it provides optimal incentives for inequity averse team members to exert effort (Bartling & von Siemens, 2010), but fails to optimally insure team members against income risk (Wilson, 1968) and may hinder efficiency by inducing teams to remain too small and homogeneous (Farrell & Scotchmer, 1988; Kräkel & Steiner, 2001).

For our purposes it is important to note that the literature on profit-sharing teams has focused on the problem of team production. Such teams, however,

¹⁴ The literature cited in this paragraph uses the term “partnerships” to describe such profit sharing teams. It models the sharing rule as unaffected by price setting. In this paper we speak of “partnerships” as teams splitting profits according to a rule unaffected by price setting behavior of team members. In contrast, we use the term “consortia” for teams splitting profits according to ratios affected by price setting behavior of team members.

often compete with other teams for profits. Nonetheless, the literature has given little attention to the relation between teams' internal structure (e.g., the specific way by which profits are shared) and the way that inter-team competition unfolds, and even less to situations where competing teams differ in their internal organization.

Dechenaux, Kovenock, and Sheremeta (2012, p. 3) provide an extensive survey on experimental work on contests, which they define as situations in which "competing agents have the opportunity to expend scarce resources – such as effort, money, time, or troops – in order to affect the probabilities of winning prizes". The vast majority of work mentioned in this survey considers agents as individual decision makers, examining contest features ranging from the number and heterogeneity of players, spillover and externalities, and length of play, to sabotage, collusion, communication, and alliance formation (to name a few).

There is however, a small strand of literature on contests that considers groups, rather than individuals, as the competing agents. A prevailing result is that contests between groups can serve to increase efforts and mitigate within-group free riding (Cason, Sheremeta, & Zhang, 2012; Leibbrandt & Sääksvuori, 2012; Nalbantian & Schotter, 1997; Sutter & Strassmair, 2009). Bornstein, Kugler, Budescu, & Selten (2008) and Abbink, Brandts, Herrmann, & Orzen (2010) compare contests between individuals to contests between groups. The latter additionally examine asymmetric contests between individuals and groups.

Very few papers examined the way competing groups are organized. Abbink et al. (2010) studied the effect of intra-group punishment in an inter-group Tullock contest. Bornstein & Gneezy (2002) and Bornstein et al. (2008) both compare inter-consortia to inter-partnership Bertrand price competition. These three papers compared symmetric contests/markets, where both groups are organized in the same way. Recent un-published work by Cason, Sheremeta, & Zhang (2014) is the only one we are aware of to experimentally explore an *asymmetric* contest between groups that differ in their internal organization, with intra-group communication available in one group but absent in the other.

The two related papers by Bornstein & Gneezy (2002) and Bornstein et al. (2008) focus on how the presence of conflicting interests within groups affects the outcome of a Bertrand price competition by experimentally comparing competitions between partnerships, where intra-group conflict is absent, to competitions between consortia, where intra-group conflict prevails. Both papers find that competitions between partnerships yield lower prices than competitions between consortia, convincingly demonstrating that the sharing rules

that govern profit distribution among members of the winning team matter and should be considered seriously by both market regulators and competing teams.

We broaden the analysis of teams' internal organization in inter-group contest, in particular their profit sharing rules, in two crucial dimensions. First, we allow for heterogeneity in profit-sharing rules. Our experimental contests are not composed only of symmetric pairs of consortia or partnerships, but can also be asymmetric, including one consortium and one partnership competing with each other. Second, the notion of heterogeneous competitions de-trivializes the epistemic nature of the contest, in the sense that it is no longer obvious that members of one team know how the other team is organized (i.e., whether the competitor is a consortium or a partnership). To the best of our knowledge we are the first to experimentally address the epistemic state of team members vis-à-vis a competing team, by examining how the (lack of) knowledge about the competitor's sharing rule affects the outcome of the competition.

2. *Experimental design and procedure*

As experimental paradigm we use a Bertrand duopoly game abstracting from production and trade, in which each of the two competitors consists of a team of three players. The game was introduced by Dufwenberg and Gneezy (2000) for individual players, and modified as a team game by Bornstein and Gneezy (2002). In every period of the game, each member $k \in \{1,2,3\}$ in team $i \in \{1,2\}$ simultaneously states an individual asking price $X_{ik} \in \{2,3,\dots,25\}$. The total asking price of team i is denoted by X_i ($X_i = \sum_{k=1}^3 X_{ik}$). The team with the lower total asking price wins the competition; if the two total asking prices are equal there is a tie.

There are two types of teams, differing by how profits are divided among the three team members. Under an individualistic *consortium* (C) structure, each team member is paid her individual asking price if the team wins, and half her asking price in case of a tie. Under an egalitarian *partnership* (P) structure each team member receives the average asking price if the team wins, and half the average asking price if there is a tie. In both sharing rule types members of the losing team receive nothing. The payoff of member k in team i is given by

$$\pi_{ik}^C = \begin{cases} X_{ik}, & X_i < X_j \\ X_{ik}/2, & X_i = X_j \\ 0, & X_i > X_j \end{cases}$$

for members of consortia, and by

$$\pi_{ik}^P = \begin{cases} X_i/3, & X_i < X_j \\ X_i/6, & X_i = X_j \\ 0, & X_i > X_j \end{cases}$$

for members of partnerships.

Table 1 illustrates the experimental setup. We varied the composition of the market in terms of the competing teams' sharing rules, and the transparency of these sharing rules. This results in two types of homogeneous markets where a consortium is matched with another consortium (CC), or a partnership is matched with another partnership (PP), and in heterogeneous markets where consortia are matched with partnerships (CP). Participants always had information about the sharing rule type of their own team. In the *transparency* (t) treatments participants were also informed about the sharing rule of the competing team; in the *intransparency* (i) treatments they were not.

	transparent (t)	intransparent (i)
Homogeneous Markets		
<i>Two consortia (CC)</i>	CCt (N=96; N _m =16)	CCi (N=96; N _m =16)
<i>Two partnerships (PP)</i>	PPt (N=90; N _m =15)	PPi (N=96; N _m =16)
Heterogeneous Markets		
<i>One consortia, one partnership (CP)</i>	CPt (N=96; N _m =16)	CPi (N=96; N _m =16)

Table 1: Experimental Treatments. N=number of participants; N_m=number of markets.

The interaction was repeated for 120 announced periods. The team's composition, sharing rule, and corresponding competitor were determined randomly before the first period and remained constant over all 120 periods. After every period participants received feedback about their own asking price, the total asking price of their team, the total asking price of the other team, their earnings in the period, and their cumulative earnings. Before starting, paper instructions were distributed to all participants in a session. The instructions informed the participants about the two available sharing rules, the time horizon, and the

information they will receive on their computer screens during the experiment. The paper instructions for the “homogeneous” and “heterogeneous” treatments were identical; the difference between the “transparent” and “intransparent” treatments was only a single word.¹⁵ There was no possibility of communicating neither within teams nor between teams. The experiment was computerized using the software z-tree (Fischbacher, 2007) and participants were recruited from a pool of more than 5000 people using ORSEE (Greiner, 2004). Overall, 570 participants took part in 24 experimental sessions at the Bonn EconLab. The average session lasted about 90 minutes and participants earned about €17 on average. Individual payoffs ranged from €3.50 to €47.

3. Theoretical considerations and previous findings

3.1 Nash equilibrium

The unique Nash equilibrium of the stage game is that all participants demand the lowest possible individual asking price, $X_{ik}=2$. This is true regardless of the team’s sharing rule type (for a more detailed discussion see Bornstein et al., 2008). Furthermore, the Nash equilibrium is not affected by the competitor’s sharing rule or by the knowledge thereof. Thus, the standard game theoretic prediction for the stage game is the same for all our treatments. Since the fact that the game will be played repeatedly for exactly 120 periods was made known to the participants, by backward induction it follows that the stage game equilibrium holds for each period of the repeated game as well.

3.2 Previous results: individual adaptation in homogeneous markets

Previous results, however, indicate that teams’ sharing rules influence behavior, even when they do not affect the Nash equilibrium. Both Bornstein and Gneezy (2002) and Bornstein et. al. (2008) found more tacit collusion (higher prices) in Consortia markets than in Partnership markets.

Bornstein and Gneezy (2002) provide a compelling argument, based on a simple process of individual adaptation, to predict and explain this result. A slightly altered version of their argument is as follows: suppose member k in team i is undecided between a pair of possible prices, \bar{X}_{ik} and \underline{X}_{ik} , with $\bar{X}_{ik} > \underline{X}_{ik}$ and $\Delta = \bar{X}_{ik} - \underline{X}_{ik}$. The decision’s implication on the team’s winning probability is similar whether it is a consortium or a partnership – the probability of winning

¹⁵ See Appendix for the paper instructions.

is higher when the asking price is lower. However, consortia and partnerships differ in the way the choice between \bar{X}_{ik} and \underline{X}_{ik} affects the profit of the decision maker herself (member k in team i). Consortia provide a weaker incentive to bid the lower price (\underline{X}_{ik}): if the team wins when \underline{X}_{ik} was chosen, member k earns Δ less than what she would earn had she chosen \bar{X}_{ik} . In a partnership k 's earnings are decreased by only $\Delta/3$. Thus, a team member's inclination to lower her asking price, at a private cost to herself, in order to increase the team's chances of winning is lower in consortia than in partnerships. Similarly, the temptation to increase personal profits at a cost to the team's chance of winning is higher in consortia than in partnerships.

The above argument applies to our homogeneous markets as well, and does not seem to rely on the transparency of the sharing rules. Therefore we expect to find higher prices in homogeneous partnerships markets as compared to consortia markets, for both transparent and intransparent markets.

Bornstein and Gneezy (2002) and Bornstein et. al. (2008) applied the individual adaptation argument outlined above to homogeneous markets, but it is relevant to heterogeneous markets as well. Regardless of whether the competing team is a consortia or a partnership, members of partnerships have a stronger incentive to opt for lower prices, increasing their teams' probability of winning. The resulting prediction is that partnerships will have a competitive edge over consortia when competing against each other in the same market – they will win the competition more often.

Even if partnerships indeed win the competition more often, predicting *price levels* in heterogeneous markets is not obvious. Three scenarios come to mind:

- (1) low (partnership) prices: in an attempt to compete with partnerships, consortia will be forced to lower their asking prices to partnership levels, resulting in prices similar to those in homogeneous partnerships prices.
- (2) high (consortia) prices – partnerships will seize the opportunity to enjoy higher prices, and will increase their prices to consortia levels.
- (3) intermediate prices – both processes will take place simultaneously, resulting in prices that are higher than homogeneous partnerships

prices, lower than homogeneous consortia prices, but different from both.

Learning models provide a possible source for deriving predictions in our settings. There are two distinct families of learning models: stimulus learning on the one hand and belief based learning on the other. Stimulus learning is inspired by Thorndike’s “Law of Effect” (Thorndike, 1898) – the likelihood of repeating a specific choice rises after that choice has led to a good outcome. Its most prominent representative is the reinforcement learning model (Roth and Erev, 1995; Erev and Roth, 1998). Predictions derived from reinforcement learning were very much in line with the results of Bornstein and Gneezy (2002) and Bornstein et al. (2008). To apply reinforcement learning to our setup, we assume that before the first period participants are completely ignorant about which asking price to state. Every price in the feasible set $X_{ik} \in \{2, 3, \dots, 25\}$ is equally likely to be chosen. If the (randomly) chosen price yields a profit, the propensity of choosing the same price again increases, and the increase is proportional to the profit.¹⁶

We derived the predictions of reinforcement learning by simulating the behavior of 12600 virtual players in 2100 experimental markets. The simulations show that in homogeneous markets *individual asking prices* increase throughout the repeated interaction in consortia and decrease in partnerships. In heterogeneous markets the pattern is the same, but the difference between consortia and partnerships within the same (heterogeneous) market is smaller than the difference between the homogeneous markets. It follows that according to reinforcement learning, *market prices* in homogeneous consortium markets are higher than in homogeneous partnership markets, and that prices in heterogeneous markets lie in between (and are rather stable throughout the 120 periods).¹⁷

¹⁶ We use the same parameters as Bornstein and Gneezy (2002) and Bornstein et al. (2008). Initially, every price between 2 and 25 has a weight of 10. After every period the profit earned in that period is added to the weight of the price played. For example: Subject i plays in a consortium. In the first period i randomly chooses to bid 8, i ’s team wins, and i earns 8 points. The updated weight for choosing 8 in the second round is 18 for 8, and the other weights remain unchanged and equal 10.

¹⁷ See Appendix B for more details.

Belief based learning presumes more sophisticated players than stimulus learning. Whereas in stimulus learning players only need to have memory, belief based models additionally assume that players are able to maximize their (expected) payoffs. The most prominent belief based learning model is fictitious play (Brown 1951): a player chooses the action that maximizes her expected payoffs based on her beliefs about the future actions of the other players. Beliefs about the other players' future actions are derived from their past actions. Specifically, the more often player j has chosen action X in the past, the higher the probability that player i attaches to player j choosing X in the future. If, for instance player j choose action X in k out of t past periods, player i 's belief of j choosing X in the next period would be $P=k/t$. In the first period, there is no history of past actions, so players pick randomly from the set of feasible actions $X_{ik} \in \{2,3,\dots,25\}$. As of the second period, players start best responding to the expected actions of the other players.¹⁸

We simulated the behavior of 360 virtual participants in our experimental markets. Similar to reinforcement learning, fictitious play predicts prices in CCT to be high, in PPT to be low and in CPT to be in between. Counter to Reinforcement learning it predicts that collusion is declining in all treatments over time, thus gradually approaching the Nash Equilibrium. Moreover it predicts that the influence of the other team's sharing rule on an individual's bidding is a lot stronger than it would be according to the prediction of reinforcement learning.¹⁹

Both reinforcement learning and fictitious play are mute with respect to the effect of (in)transparency on behavior. To the best of our knowledge there are no learning models or game theoretic solution concepts that are sensitive to the knowledge players have about the payoff function of other players. In the absence of such models/solution concepts, a straight forward prediction is that tacit collusion on high prices is more likely to occur in transparent markets. This prediction is based on the intuition that (1) tacit collusion requires that members of each team accurately predict the behavior of members of the other team, and (2) such predictions are more accurate when there is more information about the other team.

¹⁸ If there is more than one best responding action, each is equally likely to be picked.

¹⁹ See Appendix C for more details.

4. Results

We first report treatment effects on average market prices and subsequently consider individual learning behavior.

4.1 Treatment Effects

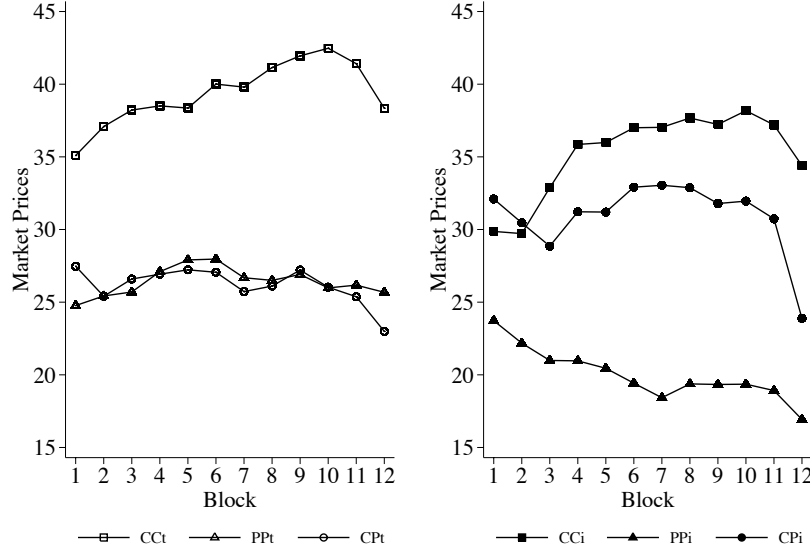


Figure 1: Mean market prices in transparent (left) and intransparent (right) markets

Market prices denote the total asking prices of the winning teams in every period. For better visibility, the 120 periods are pooled into 12 blocks of 10 periods each. CCt has 96 subjects (16 markets), PPt 90 (15), CPt 96 (16), CCi 96 (16), PPi 96 (16), and CPI 96 (16).

Result 1. *Prices are higher in homogeneous consortium markets than in homogeneous partnership markets, both with and without transparency..*

The large difference between the CCt and the PPt treatment in the left panel of Figure 1 (Mann-Whitney, $p < 0.001$, $N = 31$) confirms the previous findings of Bornstein and Gneezy (2002) and Bornstein et al. (2008). As can be seen in the right panel of Figure 1, the effect is equally pronounced when the other team's sharing rule is unknown (CCi and PPi; Mann-Whitney, $p < 0.001$, $N = 32$). This shows that the differences in market prices between consortia

markets and partnership markets do not depend on the knowledge of the competitor's sharing rule.

Result 2. *When sharing rules are transparent, prices in heterogeneous markets are as low as in homogeneous partnership markets.*

As can be seen in the left panel of Figure 1, price levels in CPt markets are virtually identical to those in PPt markets (Mann-Whitney, $p > 0.87$, $N=32$), and significantly lower than prices in CCt markets (Mann-Whitney, $p < 0.0005$, $N=32$).

Result 3. *When sharing rules are not transparent, prices in heterogeneous markets are (almost) as high as in homogeneous consortia markets.*

As can be seen in the right panel of Figure 1, price levels in CPi markets are higher than those in PPI markets (Mann-Whitney, $p < 0.001$, $N=32$), and not significantly different from prices in CCI markets (Mann-Whitney, $p = 0.152$, $N=32$).

Result 4. *Transparency leads (a) to higher prices in homogeneous markets but (b) to lower prices in heterogeneous markets.*

When both competitors in the market have the same sharing rule, prices are higher when sharing rules are transparent. This is the case for consortia markets (Mann-Whitney, $p < 0.05$, $N=32$) and partnerships alike (Mann-Whitney, $p < 0.05$, $N=31$). However, the opposite happens when competitors have different sharing rules. In this case, prices are lower when sharing rules are transparent (Mann-Whitney, $p < 0.05$, $N=32$).

Result 5. *Partnerships have an advantage over consortia in heterogeneous markets – they ask for less, win more often and accumulate more wealth.*

When facing each other in heterogeneous markets partnerships ask for lower prices than consortia in both transparent and intransparent markets, but the differences are not significant (Mann-Whitney, $p=0.407$, $N=32$; $p=0.14$, $N=32$; transparent and intransparent markets, respectively). Still, partnerships win more often (Wilcoxon signed rank: $p = 0.007$, $p=0.0008$; transparent and intransparent markets, respectively), and this tendency increases over time – the (spearman) correlation between the (10-period) block and the winning proportion is 0.14 ($p=0.057$) for transparent markets and 0.17 ($p=0.016$) for intransparent markets. When they do win the competition, consortia do so at higher prices than partnerships in transparent markets ($p=0.03$), but the differ-

ence is not enough to offset the lower frequency of winning, and partnerships earn more (Mann-Whitney, $p=0.046$, $N=32$). When incentive structures are intransparent winning prices of consortia and partnerships are similar ($p=0.98$) and partnerships (obviously) earn more (Mann-Whitney, $p=0.0004$, $N=32$).

4.3. Learning

Result 7. *There are no differences between the treatments in period 1.*

The observed differences between our treatments can be a result of participants' prior beliefs and expectations about the market they are operating in, learning and adaptation during the repeated interactions, or both. If prior beliefs and expectations play a role in shaping the behavioral differences between the various treatments, at least some differences should be observed already in the very first period. This, however, is not the case. Asking prices in the first period are not different between the treatments (Kruskal-Wallis: $p=0.83$), indicating that learning and adaptation, and not prior beliefs and expectations, are the source of the behavioral differences between the treatments.

When we further compare the learning dynamics from the two learning models with our findings it turns out that the lower degree of sophistication notwithstanding, reinforcement learning fares a little better than fictitious play.

Fictitious play predicted a falling trend for both forms of homogeneous markets and for the heterogeneous markets. Reinforcement learning predicted a falling trend for homogeneous markets of partnerships and for heterogeneous markets. The rate of the decreasing trend was predicted to be smaller in heterogeneous markets than in homogeneous markets of partnerships. For homogeneous markets of consortia reinforcement learning predicted a rising trend – at least for the first 120 rounds. In fact a simulation of 10,000 rounds reveals that ultimately individual asking prices start to fall even in the homogeneous markets of consortia.

Our findings show that the two learning models are right in their prediction that prices display a falling trend in homogeneous markets of partnerships under intransparency. Market prices in homogeneous markets of partnerships clearly fall under intransparency. Under transparency, however, a falling trend is not clearly visible from the graph. In the four other treatments we observe a clear endround effect in the last block of rounds where collusion gets harder to sustain because the backward induction logic gets more salient as the end of the game approaches. Until that endround effect we observe a relatively stable level of prices in the heterogeneous treatments and an increasing trend in the

homogeneous markets of consortia. Both is predicted by reinforcement learning while it is not predicted by fictitious play.

5. Discussion

In this paper we have experimentally investigated the effects of heterogeneity and transparency in sharing rules on behavior in Bertrand duopoly markets where each competitor in the market is a 3-person team. Our main result is that the outcome of heterogeneous markets crucially depends on the availability of information about the competing team's sharing rule type. When sharing rules are transparent, heterogeneity leads to low market prices, similar to those of homogeneous partnership markets. When sharing rule types are intransparent, heterogeneity leads to high prices, near those of homogeneous consortium markets. This pattern of results is not predicted by any of the models we considered – Nash, reinforcement learning, or fictitious play.

In homogeneous markets, prices were higher when sharing rules were transparent than when they were intransparent, suggesting that knowing that the partner is similar makes it easier to tacitly collude. Within each transparency condition prices were higher when both competitors were consortia than when they were both partnerships. These results replicate those obtained by Bornstein and Gneezy (2002) and Bornstein et. al. (2008), and add to their robustness by showing that also they hold in our settings as well: a finite repeated game between fixed groups.

With respect to heterogeneous markets, we expected prices to lie in between prices of the two relevant (in terms of transparency) homogeneous markets. It was not clear, however, if prices will converge towards the lower price levels of homogeneous partnership competitions, or towards the higher prices levels of homogeneous consortium competitions. Interestingly, the answer crucially depends on the transparency of sharing rule types: When sharing rules are transparent consortia lower their prices to the level of partnerships, and when sharing rule types are intransparent it is the partnerships that adapt prices upward to consortia levels.

Differences in the salience of within- and between-team conflicts among consortia in each of the transparency conditions could be an explanation for this somewhat surprising result. Members of partnerships face only a between-team conflict between their team and the competing team. Within-team conflict is eliminated by the equal division of profits between team-members. Members of consortia face both types of conflict, which have opposing behavioral impli-

cations. Whereas in the between-team conflict it is best to ask for a low price in order to outbid the other team and win the price competition, in the within-team conflict one is tempted to ask for a high price in order to reap a higher profit should the team win.

When members of a consortium are not aware of the competing team's sharing rule (intransparency), they pay more attention to the internal conflict within their own team, which implies high individual asking prices, and focus less on competing with the other team, which implies low individual asking prices, as compared to the case when they are aware of the competing team's sharing rule type (transparency). Such behavior enables members of the competing partnership to increase their own asking prices to just below those of the consortium, such that they still win the majority of the competitions, but at a higher price, and thus increase their earnings. Under transparency the between-team conflict becomes more salient, so consortia members are driven to lower their individual asking prices in order to successfully compete with the other team, and the result is a market with low (partnership level) prices.

Our results are informative to both regulators and market participants. We clearly identify two factors which affect prices in a non-obvious way: heterogeneity and transparency of the sharing rules. Awareness of such factors is of obvious importance to regulators. For example, prices in our experiment are highest in homogeneous transparent consortia markets, indicating that such markets are particularly prone to tacit collusion; market regulators might want to pay special attention to markets with similar characteristics. Another example is the design of public procurement auctions. According to our results, prices can be reduced by inviting only partnerships (and not consortia) to take part in the auction. If the market is necessarily heterogeneous (includes both partnerships and consortia), the designer can consider forcing all competitors to reveal their sharing rule type, since our results indicate that in heterogeneous markets prices are lower with transparency.

Our results also offer important insights for market participants. Which sharing rule type should they employ? Should they reveal it to their competitors? What kind of market should the team compete in? Suppose that two teams compete in a Bertrand duopoly similar to the one in our experiment, that they can decide at the outset on a sharing rule for the team, and that that sharing rule will remain in effect throughout the repeated interaction. The meta-game played between the teams at the initial sharing rule type decision – using our results to predict the outcome of the repeated price competition – is described in Table 2. The numbers in each cell are the mean cumulative earnings of sin-

gle team members of the respective teams. For example, if sharing rules are transparent and both teams select to be consortia, each member of both teams is expected to earn 790. If sharing rules are intransparent and one team chooses to be a consortium and the other to be a partnership, each member of the consortium is expected to earn 480, and each member of the partnership is expected to earn 760.

The resulting meta-games are critically influenced by the presence or absence of transparency. When sharing rules are transparent the resulting meta-game is a stag hunt game (i.e., assurance or coordination game). This game has two pure strategy Nash equilibria – either both players choose to divide profits as consortia or both choose to be partnerships, with the (Consortia, Consortia) equilibrium being both payoff ($790 > 530$) and risk ($790 + 460 > 590 + 530$) dominant. The payoffs of the meta-game when sharing rules are intransparent correspond to a chicken game, with two non-symmetric equilibria, where one team is a partnership and the other is a consortium (i.e., the market is heterogeneous).

How can the above analysis inform market participants? The attractiveness and high profits of the (Consortia, Consortia) equilibrium in the transparent case, as opposed to the conflictual nature of the chicken game in the intransparent case, suggests that they should prefer to operate in markets with transparent sharing rules, and that once they do, they should prefer consortia-like sharing rules, reasonably expecting that the opposing team will also choose to be a consortium. If the market is intransparent there is a serious coordination problem, and it is not easy to make a recommendation, as both options (consortium or partnership) can yield either high or low outcomes, depending on the opponent's choice. Another decision a team might be required to make is in which type of market to compete (transparent or intransparent) in case the team's sharing rule is fixed. The answer to this question crucially depends on the distribution of the two sharing rules across the different markets, but generally it is clear that, regardless of one's own sharing rule, competing with a consortium is preferable to competing with a partnership.

In our experiment we assumed that the transparency or intransparency of the sharing rules is a characteristic of the market, and accordingly both teams in each market are always in the same transparency condition. In real world markets it is reasonable to assume that a team can decide whether to make its sharing rule transparent or not, irrespective of the decision of the other team. A question that we cannot answer with our data is whether it makes sense for a team to unilaterally change its transparency policy, as this can result in asym-

metry with respect to transparency of sharing rule types, which we did not examine.

The internal organization of the decision making unit is often overlooked in the study of economics decisions. Here we went beyond the small body of previous work, which has established that the internal organization can be very important in shaping the market outcome, and showed that heterogeneity and transparency of sharing rule types not only matter, but interact in an interesting, non-trivial way. Our discussion of the two meta-games that result from the two transparency conditions and the implications of our results to regulators, and especially to market participants, suggest that an interesting avenue for further research is to allow for teams' sharing rules and transparency policies to be endogenous, rather than exogenously pre-determined as in our experiment. Another route for further studies is to increase the number of competitors in the market. As long as only one competitor wins the competition, it seems reasonable to predict that the existence of at least two partnerships in the same market will lead to low prices (as in homogeneous partnership markets) regardless of the number of Consortia in the market and of the transparency condition, because the inter-partnerships competition will drag prices down. Finally, it would also be of much interest to examine the effects of communication, both within and between teams, on the unfolding of market prices in the different combinations of sharing rules and transparency that we have considered.

Transparent	Consortium	Partnership	Intranspar.	Consortium	Partnership
Consortium	790	590	Consortium	710	760
Partnership	460	530	Partnership	480	400

Table 2: Meta Game in Choosing the Nature of the Sharing Rule Endogenously: Values are average cumulative earnings of a team member in the relevant condition over 120 periods. Bold frames indicate the Nash equilibria.

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7. Appendix A: Paper Instructions

General instructions for participants

Welcome to our experiment!

If you read the following explanations carefully, you will be able to earn a substantial sum of money, depending on the decisions you make. It is therefore crucial that you read these explanations carefully.

During the experiment there shall be absolutely no communication between participants. Any violation of this rule means you will be excluded from the experiment and from any payments. If you have any questions, please raise your hand. We will then come over to you.

In any event, you will receive a lump sum of 2 euro for taking part in the experiment.

During the experiment we will not calculate in euro, but instead in points. Your total income is therefore initially calculated in points. The total number of points you accumulate in the course of the experiment will be transferred into euro at the end, at a rate of

35 points = 1 euro.

At the end you will receive from us the 2 euro plus the **cash** sum, in euro, based on the number of points you have earned.

Experiment procedure

The experiment consists of **120 periods**.

Prior to the first period, the 24 participants are randomly divided into 8 groups. Each **group** has **3 members**. The same 3 members hence remain in the same group for the entire 120 periods. In addition, each group is randomly assigned to another group. The **other group** that is assigned to yours also remains unchanged for the entire 120 periods. Since the experiment is completely anonymous, you have no possibility of finding out who belongs to your group and who belongs to the other group.

Before the first period begins, the computer will randomly allocate one of two possible **distribution keys** to every group (we will explain below in further detail what these distribution keys look like). The distribution key remains the same for all periods.

At the beginning of each period, you may demand any number of points between 2 and 25 (**individual asking price**). As soon as all participants have stated their individual asking prices, the computer will add up your group's 3 individual asking prices and calculate the **total asking price** of your group. Similarly, the computer will also add up the other group's 3 individual asking prices and calculate the other group's total asking price. The computer will then **compare** the total asking price of your group to the total asking price of the other group.

Distribution key A:

1. If the **total asking price** of your group is lower than the total asking price of the other group, then each member of your group receives exactly the number of points he or she demanded (**individual asking price**).
2. If the **total asking price** of your group is higher than the total asking price of the other group, then each member of your group receives exactly **0 points**.
3. If the **total asking price** of your group is equal to the total asking price of the other group, then each member of your group receives exactly half the points he or she demanded (**half of the individual asking price**).

Distribution key B:

1. If the **total asking price** of your group is lower than the total asking price of the other group, then each member of your group receives exactly one-third ($1/3$) of the total asking price (**total asking price divided by 3**). In other words, the total asking price of your group is evenly distributed among all 3 group members.
2. If the **total asking price** of your group is higher than the total asking price of the other group, then each member of your group receives exactly **0 points**.
3. If the **total asking price** of your group is equal to the total asking price of the other group, then each member of your group receives exactly one-sixth ($1/6$) of the total asking price (**total asking price divided by 6**). In other words, the total asking price of your group is initially halved and then evenly distributed among all 3 group members.

The following table summarizes the two distribution keys once again:

	<i>Distribution key A</i>	<i>Distribution key B</i>
1. The total asking price of your group is lower than the total asking price of the other group.	You receive exactly your individual asking price.	You receive 1/3 of your group's total asking price.
2. The total asking price of your group is higher than the total asking price of the other group.	You receive nothing.	You receive nothing.
3. The total asking price of your group is equal to the total asking price of the other group.	You receive exactly half of your individual asking price.	You receive 1/6 of your group's total asking price.

You will be informed, on your computer screen, which distribution key your group has **and** **but not**²⁰ which distribution key the other group has. As mentioned above, during the entire 120 periods, the distribution key always remains the same.

²⁰ “and” vs. “but not” is the only difference between the “transparent” condition and the “intransparent” condition.

At the end of each period, you will be given information on:

- (a) your individual asking price
- (b) your group's total asking price
- (c) the other group's total asking price
- (d) the number of points earned by you in this period
- (e) the total number of points earned by you up to now, including this period.

8. Appendix B: Reinforcement Learning

Figure B1 shows the simulated development of individual asking prices (right) over time by treatment under the assumptions of reinforcement learning. In contrast to the Nash solution, reinforcement learning predicts a positive amount of collusion in the market. Reinforcement learning predicts prices in CC to be high, in PP to be low and in CP to be in between. Members of *consortia* teams are predicted to ask higher prices than members of *partnership* teams. The model predicts the different transparency conditions to have no effect whatsoever. These predictions are independent of the transparency condition why.

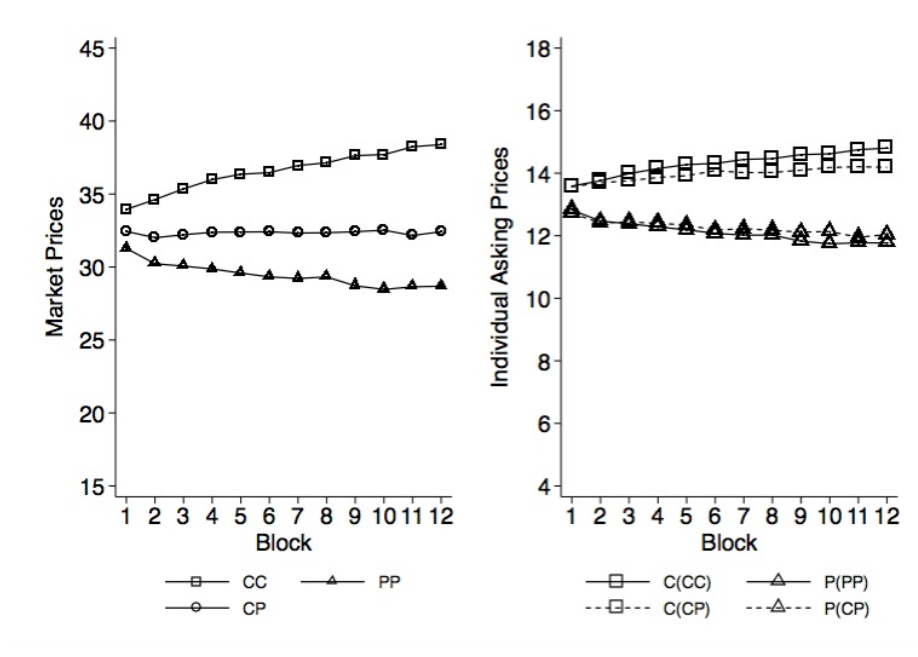


Figure B1: Left: Market Prices by Treatment as Predicted by Reinforcement Learning. Right: Individual Asking Prices as Predicted by Reinforcement Learning.

Market prices denote the total asking prices $\sum_k X_{ik}$ of the winning teams in every period. We simulated 12000 artificial individuals in 2000 markets. Because the simulation is insensitive to transparency we drop the indication “t” for transparency and “i” for intransparency. C(CP) indicates a consortium in a heterogeneous market. And C(CC) a consortium in a homogeneous market.

9. Appendix C: fictitious play

Figure C1 shows the simulated development of market prices (left) and individual asking prices (right) over time by treatment under the assumptions of fictitious play. Again, we summarize the 120 periods into 12 blocks of 10 periods each. Similar to reinforcement learning it predicts prices in CC to be high, in PP to be low and in CP to be in between. Counter to Reinforcement learning it predicts that collusion is declining in all treatments over time. Moreover, it predicts that not only the sharing rule type of one's own team influences the individual asking prices but also that of the competing team. Members of *consortia* teams are predicted to ask higher prices when competing against another consortium than when competing against a partnership and members of *partnership* teams are predicted to ask lower prices when competing against another partnership than when competing against a consortium (figure C1, right). The model predicts the different transparency conditions to have no effect whatsoever.

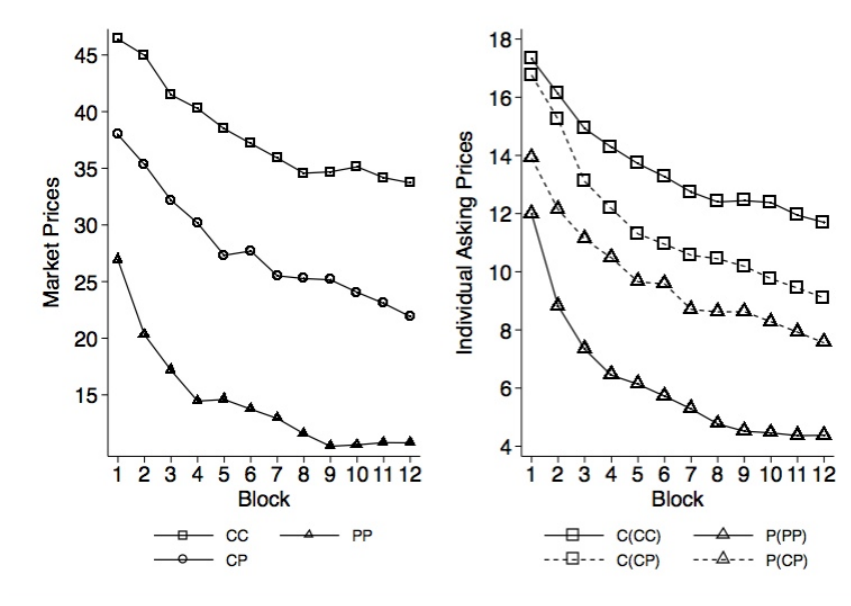


Figure C1. Left: Market Prices by Treatments as Predicted by Fictitious Play. Right: Individual Asking Prices as Predicted by Fictitious Play.

Market prices denote the total asking prices $\sum_k X_{ik}$ of the winning teams in every period. The 120 periods are pooled into 12 blocks of 10 periods each. We simulated 360 artificial individuals. Because the simulation is insensitive to transparency we drop the indication “t” for transparency and “i” for intransparency. C(CP) indicates a consortium in a heterogeneous market. And C(CC) a consortium in a homogeneous market.

10. Appendix D: Data Structure of the Experiment

Table D1 gives an overview of the data collected in the experiment.

	Partici- pants	Competitors	Markets	Periods
<i>CCt</i>	96	32	16	120
<i>PPt</i>	90	30	15	120
<i>CPt</i>	96	32	16	120
<i>CCi</i>	96	32	16	120
<i>PPi</i>	96	32	16	120
<i>CPi</i>	96	32	16	120

Table D1. Data Structure of the Experiment

Note: Every competitor is a team of 3 participants. Every market consists of 2 matched teams.

V. General Summary and Discussion

With regard to **chapter II** we conclude that loyalty rebates lead to non-rational buying behavior, amounting to an additional psychological switching cost that can cause substantial financial losses for consumers. This effect increases the potential of loyalty rebates to be used as a tool to foreclose markets and provides an argument for a more restrictive position towards loyalty rebates under consumer protection law. Previous arguments and rulings concerning the regulation of loyalty rebates under antitrust law both in the EU and in the US were mainly based on the assumption of rational buying. Stickiness effects add to these existing problems. Therefore the potential danger of loyalty rebates has been underestimated. The demonstrated stickiness effect backs the role psychological effects already play in European antitrust law today. It generally supports the greater scrutiny loyalty rebates have recently been subject to both in the EU and the US.

To the best of my knowledge, the experiment in **chapter III** is the first to show that in the realm of realistic beliefs social closeness – implemented here as shared group identity – determines how strongly senders' second order beliefs influence the amounts sent in a dictator game. I used a minimal group paradigm to induce a shared identity and reinforced it slightly. The total intervention is extremely faint. Therefore, the effect is likely to be a lot stronger in the field, where relationships are based on family ties, friendship, co-workership, and the like. Also being class mates in an MBA program (Reuben et al., 2009), exchanging a one page free text letter (Gary Charness & Dufwenberg, 2006) or being parties to a promise arising in a computer chat (Vanberg, 2008) are protocols that are likely to induce stronger shared identity than my treatment manipulation.

My results clarify that guilt aversion will make better predictions in contexts of social closeness (families, friendships, co-workers) than in anonymous contexts (anonymous market transactions). On the one hand, they reveal that experiments in a very anonymous setting may be the wrong test bed to test theories of second order belief dependent preferences. On the other, my results suggest that theories on guilt aversion should spell out that social closeness is crucial for the effect of second order beliefs on action.

While so far different authors had proposed elevated second order beliefs in ingroup interactions as a cause for ingroup favoritism (Güth et al., 2009; Ockenfels & Werner, n.d.), I show in this experiment that their explanation may at least be incomplete. Shared group identity may translate into elevated

second order beliefs but I show that it leads to a stronger influence of these beliefs. Both effects together may just reinforce each other. But according to my results ingroup favoritism could also arise under identical second order beliefs ingroup and outgroup simply because these identical second order beliefs influence action more strongly ingroup than outgroup.

From my results it appears plausible that people hold a promise to a stranger because the promise creates a shared identity between the two, causing second order beliefs to induce action. In future research, the explanation of promise keeping on the grounds a theory of guilt aversion amended along these lines should be tested against the explanation that people have a preference to hold a promise. Promises that activate guilt aversion by creating a relationship between the parties would be compatible with a theory of “lexicographic promise keeping” proposed by Ederer and Stremitzer (2014).

The finding that people have a preference to conform to the expectations of someone who is socially close may have applications in the management of teams. Guilt aversion can help to coordinate team members. Communicating expectations can incite team members who are socially close. And it seems that communicating exaggerated expectations does not backfire in well-integrated teams. At least in the realm of realistic expectations the degree of social integration of a team can be used as a mediator to fine tune the influence of mutual expectations. It seems like an interesting and promising avenue for future research to enrich the investigation of the impact of social closeness on guilt aversion by the impact of social status.

Finally, my results suggest that it is worth working on a truly empathic utility function that does not merely include other agents’ payoffs into the utility function but adds more elements of their utility. A theory of other regarding reference dependent preference with expectation based reference points along the lines of Köszegi and Rabin (2006) appears to be a promising starting point.

In **chapter IV** we studied a Bertrand duopoly market of teams of three. Teams could share their profits equally (partnership) or according to the individual asking prices of team members (consortium). We varied market composition (two partnerships; two consortia; one partnership and one consortium) and the market transparency with respect to the opponent team’s way of sharing profits. To predict the level of winning prices in the experimental markets we derived Hypotheses from Nash Equilibrium, Reinforcement Learning and Fictitious Play.

Our results suggest that the variables we vary (market composition and transparency) are highly relevant in price competition between teams. Our results also suggest that participants learn over time because in the first round there is no difference between treatments and these differences only evolve over time

but the learning models (reinforcement learning and a variant of belief learning, i.e. fictitious play) perform poorly in predicting the effects of our two manipulations.

Our most striking and maybe most surprising finding is the contrast between our result 3 and our result 4. When incentive structures are transparent, prices in heterogeneous markets are as low as in homogeneous partnership markets. But when incentive structures are intransparent, prices in heterogeneous markets are (almost) as high as in homogeneous consortia markets. The second of these two results was not predicted by any of the models applied. Ex post we explain this finding by the salience of conflict. In transparent markets the detailed information a player has about the other team highlights the conflict between teams driving down prices. In partnerships on intransparent markets, most information available concerns the conflict within the team. If players concentrate on the intra team conflict this drives prices up, because the intra team conflict lets low asking prices of a team appear as something like a public good.

On top of our own findings we corroborate the finding of Bornstein et al. (2002, 2008) by replicating their finding that prices in homogeneous markets of partnerships are lower than prices on homogeneous markets of consortia in a new setting.

Our findings will be of high importance both to managers trying to find ways to sustain high prices as well as to regulators aiming at keeping prices low. The endogenous strategic use of the variables we exogenously manipulate in this study opens up many avenues for further research.

The three studies of this dissertation illustrate that law and economics is a truly interdisciplinary field in which both disciplines mutually inspire each other. Study one showed that certainly law turns to economics methodology to answer legal questions. But more importantly studies two and three illustrate that legal expertise can make a difference for economic research, too.

This dissertation is proof that interdisciplinary research is not only crucial to generate mutual understanding but that there remains creativity in interdisciplinary research. Possibly, none of the results would have been generated either without seeing the legal question or without thorough knowledge of legal traditions of thought or practice. Certainly law alone did not offer any methodology to answer the questions raised in this dissertation. On the other hand economics did not seem to provide the perspective to generate the hypotheses tested. This illustrates that interdisciplinary research is not only a process of connecting knowledge already generated but is indeed an important means of generating new knowledge.

VI. Zusammenfassung in Deutscher Sprache

Diese Dissertation illustriert Perspektiven, die einen wirklichen Austausch von Rechtswissenschaften und experimenteller Wirtschaftsforschung erlauben. Die Illustration geschieht in drei je eigenständigen Forschungsartikeln. Traditionell wird die Disziplin der Rechtsökonomik als seine Disziplin verstanden, die (mikro)ökonomische, meist theoretische Ansätze nutzt, um Rechtsfragen zu beantworten. Rechtsfragen in diesem Sinne können Rechtspolitische oder wirklich dogmatische Fragen der Rechtsauslegung sein. Die drei Kapitel dieser Dissertation zeigen, dass der Transfer zwischen Rechtswissenschaft und ökonomischer Forschung nicht derart einseitig ist, sondern die Möglichkeit einer wirklich wechselseitigen Inspiration besteht. Eine Inspirationsrichtung besteht in der Beantwortung genuiner Juristischer Fragen durch ökonomische Methoden (Kapitel 1). In die andere Richtung kann jedoch auch Jahrhundertalte juristische Intuition die Hypothesenbildung in ökonomischer Forschung beeinflussen (Kapitel 2) oder schlicht die Praxisnähe der Rechtswissenschaften helfen wichtige Variablen zu erkennen, die strategische Interaktion beeinflussen (Kapitel 3).

Wettbewerbspolitik verlässt sich üblicherweise auf die Annahme des rational agierenden Konsumenten, obwohl andere Verhaltensmodelle typischerweise Konsumentenverhalten besser vorhersagen. In drei Experimenten, die ich in Kapitel 3 berichte, untersuchen wir theoretisch und experimentell den Einfluss von sogenannten Zielrabatten auf das Entscheidungsverhalten von Menschen. Dabei stützen wir uns auf das alternative Verhaltensmodell der Cumulative Prospect Theory (CPT). CPT sagt vorher, dass Zielrabatte Konsumenten dadurch schädigen können, dass sie verhindern, dass Konsumenten in rationaler Weise vom Rabattanbieter zu einem Konkurrenten wechseln. In den Experimenten entschieden die Probanden zunächst, ob sie an einem stilisierten Rabattsystem teilnehmen wollten. Es war unsicher, ob sie die Rabattbedingung (den Kauf einer bestimmten Mindestmenge) erreichen können würden. Zielrabatte reduzierten die Wahrscheinlichkeit erheblich, dass die Teilnehmer zu einer Außenoption mit einem höheren Erwartungswert wechseln würden. Wir schließen daraus, dass Zielrabatte Konsumenten zu erheblichem Schaden erreichen können und möglicherweise ein unterschätztes Potential haben, Märkte ineffizient zu verschließen. Unser Befund begründet daher zusätzliche Argumente warum marktbeherrschenden Unternehmen die Zielrabatte nutzen ihren Markt monopolisieren könnten oder ihre Marktmacht missbrauchen. Ebenfalls

folgen aus unserem Befund Bedenken gegen Zielrabatte aus Gründen des Verbraucherschutzes.

In dem Laborexperiment, das ich in Kapitel 2 berichte, teste ich, ob guilt aversion, d.h. eine Präferenz dafür, die Erwartungen anderer Menschen zu erfüllen, sich dann starker auswirkt wenn die Agenten einander sozial nah stehen. Ich induziere zwei verschiedene Gruppenidentitäten in Teilnehmern und teile die Teilnehmer dann zufällig einem von zwei Versuchsbedingungen zu: Sender spielen entweder ein Diktatorspiel mit einem Empfänger aus ihrer eigenen Gruppe (Ingroup-Bedingung) oder mit einem Empfänger aus der anderen Gruppe (outgroup-Bedingung). Ich lasse die Sender in Abhängigkeit von ihren second-order beliefs entscheiden wie viel Geld sie an die Empfänger senden wollen. Ich finde, dass es im Bereich realistischer beliefs (d.h. der Sender erwartet, dass der Empfänger erwartet, dass der Sender nicht mehr als die Hälfte des zu verteilenden Geldes sendet) einen positive Einfluss der second order beliefs auf die Höhe des gesendeten Betrags gibt. Dieser Einfluss ist im ingroup-Bedingung stärker als in der outgroup-Bedingung. In beiden Bedingungen reagieren ungefähr die Hälfte der Sender überhaupt nicht auf second order beliefs. In der ingroup-Bedingung identifizieren sich die Sender, die nicht auf die second order beliefs reagieren weniger mit ihrer eigenen Gruppe. Das gilt nicht für die outgroup-Bedingung.

Im dritten Kapitel untersuchen wir einen Bertrand-Duopol-Markt im Labor. In diesem Markt wird jeder der beiden Wettbewerber als ein Team aus je drei Personen modelliert. Jedes Teammitglied nennt einen eigenen Angebotspreis. Die drei so genannten Preise eines Teams werden aufsummiert und bilden den Angebotspreis des betreffenden Teams. Das Team mit dem niedrigeren Angebotspreis gewinnt den Wettbewerb in der betreffenden Runde. Die Wettbewerber können eine von zwei Formen haben. In *Konsortien* erhält jedes Teammitglied, wenn sein Team gewinnt, den von ihm genannten Angebotspreis; in *Partnerschaften* erhält jedes Mitglied, wenn sein Team gewinnt, den durchschnittlichen Preis, den die Mitglieder dieses Teams genannt haben. Wir variieren die Marktzusammensetzung und die Transparenz bezüglich der Teilungsregel des anderen Teams. Unsere Ergebnisse zeigen: (1) Homogene Märkte aus Konsortien generieren substantiell höhere Preise als homogene Märkte, die nur aus Partnerschaften bestehen. Das gilt sowohl unter Transparenz als auch unter Intransparenz. (2) In transparenten Märkten generieren heterogene Märkte so niedrige Preise wie homogene Märkte aus Partnerschaften. (3) In intransparenten Märkten generieren heterogene Märkte dagegen fast so hohe Preise wie

homogene Märkte aus Konsortien. (4) Transparenz führt in homogenen Märkten zu höheren Preisen. In heterogenen Märkten dagegen führt Transparenz zu niedrigeren Preisen.

Die drei Studien, die in dieser Dissertation berichtet wurden, illustrieren, dass Law and Economics ein wirklich interdisziplinäres Forschungsfeld ist, in dem sich die beiden Disziplinen gegenseitig beeinflussen. Das erste Kapitel hat gezeigt, dass der traditionelle Ansatz, nach dem man genuine Juristische Fragen mit der Hilfe ökonomischer Methoden beantwortet auch unter den Erweiterungen der Experimentalökonomik fruchtbringend wirkt. Vielleicht noch wichtiger ist, dass die beiden Kapitel zwei und drei illustrieren, dass Rechtswissenschaftliches Wissen auch für ökonomische Fragestellungen neue Lösungen zuführen kann.

Diese Dissertation ist ein Beleg, dass interdisziplinäre Forschung nicht nur wichtig ist, um gegenseitiges Verstehen unterschiedlicher – hier sozialwissenschaftlicher – Disziplinen zu generieren, sondern dass interdisziplinäre Forschung eine eigene Quelle wissenschaftlicher Kreativität darstellt. Möglicherweise wäre keines der hier berichteten ökonomischen Forschungsergebnisse erzielt worden, ohne die gründliche Kenntnis juristischer Fragestellungen, juristischer Tradition und juristischer Praxis. Ökonomie allein schien nicht den Blick auf die Probleme bereit zu halten um die hier getesteten Hypothesen zu generieren. Zweifellos konnten andererseits juristische Methoden allein die hier untersuchten Fragen nicht beantworten. Dies alles zeigt, dass interdisziplinäre Forschung nicht nur ein Mittel ist Wissen zu verknüpfen, sondern ein Weg neues Wissen zu generieren.

VII. Ehrenwörtliche Erklärung

Mir ist die geltende Promotionsordnung der Wirtschaftswissenschaftlichen Fakultät der Friedrich Schiller-Universität Jena bekannt. Ich habe die vorliegende Dissertation selbst angefertigt und keine Textabschnitte eines Dritten oder eigener Prüfungsarbeiten ohne Kennzeichnung übernommen. Alle von mir benutzten Hilfsmittel, persönliche Mitteilungen und Quellen habe ich an den entsprechenden Stellen kenntlich gemacht. Bei der Auswahl und Auswertung des Materials sowie bei der Herstellung des Manuskriptes haben mich ausschließlich meine Koautoren unterstützt, d.h. Andreas Glöckner und Emanuel Towfigh in Kapitel 1 sowie Michael Kurschilgen und Ori Weisel in Kapitel 3. Den genauen Umfang habe ich in der Einleitung vermerkt. Ich habe nicht die Hilfe eines Promotionsberaters in Anspruch genommen. Dritte haben weder unmittelbar noch mittelbar geldwerte Leistungen von mir erhalten für Arbeiten, die im Zusammenhang mit dem Inhalt der vorgelegten Dissertation stehen. Ich habe diese Dissertation noch nicht für eine staatliche oder andere wissenschaftliche Prüfung eingereicht. Ich habe weder die vorliegende Dissertation noch eine in wesentlichen Teilen ähnliche Dissertation bei einer anderen Hochschule bzw. anderen Fakultät eingereicht. Eine andere, rechtswissenschaftliche Abhandlung habe ich unter dem Titel „(Behavioral) Law and Economics im europäischen Wettbewerbsrecht – Missbrauchsaufsicht über Zielrabatte“ am rechtswissenschaftlichen Fachbereich der Rechts- und Staatswissenschaftlichen Fakultät der Rheinischen Friedrich-Wilhelms-Universität Bonn als Promotion eingereicht. Sie wurde mit summa cum laude bewertet.

Bonn, den 16.11.2014

